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*The American Historical Review* appears in February, April, June, October, and December of each year. It is published by the American Historical Association, 400 A Street, S.E., Washington, D.C. 20003 and is printed and mailed by the William Byrd Press, 2901 Byrdhill Road, Richmond, Virginia 23228. The editorial offices are located in Ballantine Hall, Indiana University, Bloomington, Indiana 47405.

The *AHR* is sent to members of the American Historical Association and to institutions holding subscriptions. Membership dues: For incomes over \$30,000, \$40.00 annually; \$20,000-\$29,999, \$35.00; \$15,000-\$19,999, \$30.00; \$10,000-\$14,999, \$20.00; below \$10,000 and joint memberships \$10.00; associate (non-historian) \$20.00; life \$650. The proportion of dues allocated to the *AHR* is \$15.00. Subscription rates effective for volume 82: Class I, *American Historical Review* only. United States, Canada, and Mexico \$39.00, foreign \$43.00. Further information on membership, subscriptions, and the ordering of back issues is contained on the two pages—1 (a) and 2(a)—immediately preceding the advertisements.

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## The History of Contract as a Motif in Political Thought

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HARRO HÖPFL  
and  
MARTYN P. THOMPSON

THE HISTORY OF EUROPEAN CONTRACTARIAN THOUGHT has been radically distorted by certain pervasive assumptions in the established classics on the subject. As intellectual history, the story of European contractualism was created by the formidable mind of the German legal scholar, Otto von Gierke, in the late nineteenth century. Most of Gierke's career was devoted to the research and writing of his monumental *Das deutsche Genossenschaftsrecht*, whose four volumes were published in 1868, 1873, 1881, and 1913. Actually composed two decades before publication, the fourth volume contains the critical sections on contractarian thought during the period 1500–1800, which Ernest Barker published in English translation in 1934.<sup>1</sup> As amplified and disseminated by Barker and J. W. Gough, Gierke's views have become standard for our understanding of this aspect of early modern political thought. Within his framework of supposed essentials, a host of specialized studies has appeared. Although valuable in some respects, much of this work is vitiated by ahistorical dogma. We shall first attempt to uncover his dogmatic elements and then proceed to sketch an alternative story.

The first task is by no means simple, for the literature is full of contradictory assertions and judgments. Everyone agrees on the centrality of contractarian arguments to the political thoughts of the sixteenth, seventeenth, and eighteenth centuries; but, aside from consensus at this trivial level, almost every statement about the history of contract has been met with contradictions. We are told, for example, that social contract theory was—and was not—invented in ancient Greece, that the Christian Middle Ages did—and did not—rediscover or anticipate the rediscovery of social contract theory, and that the Protestant and Cath-

The authors would like to thank K. R. Minogue for many helpful suggestions.

<sup>1</sup> Gierke, *Natural Law and the Theory of Society, 1500 to 1800*, ed. and trans. Ernest Barker, 2 vols. (Boston, 1934).

olic *monarchomachi* of the sixteenth century did—and did not—invent social contract theory.<sup>2</sup>

Disputes about origins, perhaps inescapable in intellectual history, are in this instance compounded by other controversies. Opinions have differed widely about the sources and appeal of contractarian argument in the early modern period. With different emphases, in different combinations, and for varying reasons, the following possible sources have been mentioned: Old Testament history; medieval biblical exegesis; medieval references to contracts and feudal understandings of social and political relations; the teachings of the Church Fathers; the ideas of the Conciliar Movement; Lutheran and Calvinist ideas and practices; texts of the civil law; the supposed constitutional arrangements of ancient city states and the Roman Republic; the past and more recent constitutions of the German Empire, Aragon, Switzerland, Poland, and Holland; the gradual breakdown of the extended family; the rise of capitalism; the rise of individualism and of rationalism; and the impact of the scientific revolution of the seventeenth century. In the debate about the relative significance of these presumed origins, rarely has an author singled out one source for the rise of modern European contractualism.<sup>3</sup> Other disputes have focused on which writers and

<sup>2</sup> Although we do not intend to provide a comprehensive bibliography of the secondary literature that touches the history of European contractualism, we would like to present evidence of the dramatic confusions and contradictions in that literature. Several scholars have claimed that social contract theory was invented in ancient Greece; see J. W. Gough, *The Social Contract: A Critical Study of Its Development* (1936; 2d ed., Oxford, 1957), chap. 2; A. Voigt, ed., *Der Herrschaftsvertrag*, Politica, no. 16 (Neuwied am Rhein, 1965), Introduction; I. Kärst, "Die Entstehung der Vertragstheorie im Altertum," *Zeitschrift für Politik*, 2 (1909): 505–38; and Ernest Barker, *Greek Political Theory* (rev. ed., London, 1960), 64. The idea that the ancient world knew nothing of social contract theory is implicit in works dating its origin in later ages; also see, for the argument that there were no social contract systems in antiquity although a few lines of a few authors might be interpreted as referring to a *pactum societas*, though not a *pactum sujectionis*, F. Atger, *Essai sur l'histoire des doctrines du contrat social* (Nîmes, 1906), chap. 1. R. Hildenbrand argued that contractual theory arose among the Epicureans, but Otto Gierke denied that it did; see Hildenbrand, *Geschichte und System der Rechts- und Staatsphilosophie*, 1 (Leipzig, 1860): 515–17; and Gierke, *The Development of Political Theory*, trans. B. Freyd (New York, 1939), 125–26, n. 56. Gierke argued that the Christian Middle Ages witnessed the introduction of contractualist theory; Gierke, *Political Theories of the Middle Ages*, trans. F. W. Maitland (Cambridge, 1913), 39–40. But others have claimed that medieval thought was not contractualist or that its contractualism was of a very different kind from later conceptions; see J. Plamenatz, *Man and Society*, 1 (London, 1963): 98, 156; and J. Dennert, ed., *Beza, Brutus, Hotman* (Köln, 1968), Introduction, 16. For the claim that the Protestant and Catholic *monarchomachi* invented social contract theory, see H. J. Laski, ed., *Vindiciae contra tyrannos: A Defence of Liberty against Tyrants* (London, 1924), Introduction, 47; U. Scheuner, "Ständische Einrichtungen und innerstaatliche Kräfte in der Theorie Bodins," in Horst Denzer, ed., *Jean Bodin* (Munich, 1974), 379–98; J. Freund, "Quelques aperçus sur la conception de l'Histoire de Jean Bodin," in *ibid.*, 105–22; P. Mesnard, *L'Essor de la philosophie politique au XVI<sup>e</sup> siècle* (2d ed., Paris, 1952), pt. 2, chap. 6: "Les débuts de la théorie du contrat"; and Plamenatz, *Man and Society*, 155–56, 162–64. G. N. Clark has argued that social contract theory "was first revived" in the sixteenth century by the opponents of monarchy; see his *The Seventeenth Century* (1929; 2d ed. repr., Oxford, 1961), 216. Dennert, however, has argued that the *monarchomachi* were not at all concerned with a social contract and Gierke claimed that they very rarely considered a social contract; Dennert, *Beza, Brutus, Hotman*, xlv–li; and Gierke, *The Development of Political Theory*, 100–02. Ernst Cassirer rooted social contract theory in the sixteenth and seventeenth centuries, but he did not claim that the *monarchomachi* advanced it; Cassirer, *The Myth of the State* (New Haven, 1946), chap. 13: "The Renaissance of Stoicism and 'Natural Right' Theories of the State."

<sup>3</sup> Gierke and Gough have argued for the relevance of Old Testament history, medieval contractualism, the teachings of the Church Fathers, the ideas of the Conciliar Movement, the ideas and practices of the Reformed Churches, and traditions of civil law and current constitutional arrangements in accounting for the appeal and persuasiveness of early modern contractualisms; Gierke, *The Development of Political Theory*, 91–92; and Gough, *The Social Contract*, 22. Hannah Arendt, however, has denied the relevance of any of these sources except the Old Testament in accounting for the contractualism of early American Puritans; Arendt, *On Revolution* (London, 1963), 171–73. Atger and J. N. Figgis, have stressed the significance of medieval contractualisms



writings properly belong in a history of social contract. Some historians have championed Spanish natural law theorists of the sixteenth century, while others have denied that these thinkers presented a proper social contract theory at all. Similar controversies surround the ideas of Salamoni<sup>us</sup>, Théodore de Bèze, Jean Boucher, Junius Brutus (Philippe du Plessis-Mornay?), George Buchanan, Richard Hooker, Juan de Mariana, and various pamphleteers of the English Civil War. And even Hobbes, Locke, and Rousseau have had their credentials for inclusion challenged, directly or by implication.<sup>4</sup>

and feudal understandings; Atger, *Essai sur l'histoire des doctrines du contrat social*, 107–08; and Figgis, *Political Thought from Gerson to Grotius, 1414–1625: Seven Studies* (rev. ed., New York, 1960), 13, 169, 172. G. Oestreich has denied the relevance of medieval origins, insisting instead upon the singular importance of the ideas and practices of the Reformed Churches (feudal relations, in particular, had no influence); Oestreich, “Die Idee des religiösen Bundes und die Lehre vom Staatsvertrag,” in H. H. Hofmann, ed., *Die Entstehung des modernen souveränen Staates* (Köln, 1967), 137–51. For an emphasis on the importance of the teachings of the Church Fathers and texts on civil law in accounting for the appeal of contractualism in Reformation Europe, see M. d’Addio, *L’idea del contratto sociale dai Sofisti alla Riforma* (Milan, 1954); for the claim that the Conciliarists directly influenced contractualism, see Francis Oakley, “On the Road from Constance to 1688,” *Journal of British Studies*, 1 (1962): 1–32. A. J. Black, however, has rejected the influence of the Conciliar Movement; Black, *Monarchy and Community: Political Ideas in the Later Conciliar Controversy, 1430–1450* (Cambridge, 1970), 8. J. H. Franklin has underlined the importance of past and present constitutional arrangements in the Empire, Aragon, Switzerland, Poland, and Holland, as has F. S. Carney in accounting for Johannes Althusius’s contractualist and federalist ideas; Franklin, “Jean Bodin and the End of Medieval Constitutionalism,” in Denzer, *Jean Bodin*, 153; and Carney, *The Politics of Johannes Althusius* (London, 1964), Introduction. H. J. Laski has emphasized the rise of capitalism, the breakdown of the extended family, and the rise of individualism, while H. Becker and H. E. Barnes have singled out the rise of capitalism and G. H. Sabine has stressed the rise of individualism and rationalism; Laski, *The Rise of European Liberalism* (rev. ed., London, 1962), chap. 1; Becker and Barnes, *Social Thought from Lore to Science*, 1 (3d ed., New York, 1961): 378; and Sabine, *A History of Political Theory* (3d ed., New York, 1951), 367–68. The significance of modern rationalism is also central to Cassirer’s account; *The Myth of the State*, 206, 216. And J. Dennert has dwelled upon the impact of the scientific revolution of the seventeenth century in explaining the origins of modern social contract theory; *Beza, Brutus, Hotman*, xlv–xlvii.

<sup>4</sup> For the idea that the Spanish natural law theorists of the sixteenth and early seventeenth centuries were social contract theorists, see Gough, *The Social Contract*, 67; J. Kleinhappl, *Der Staat bei Ludwig Molina* (Innsbruck, 1935), 96–109; R. Wilenius, *The Social and Political Theory of Francisco Suarez* (Helsinki, 1963); and R. H. Murray, *The Political Consequences of the Reformation: Studies in Sixteenth-Century Political Thought* (London, 1926), 225–36; and, for the idea that they were not, see B. Hamilton, *Political Thought in Sixteenth-Century Spain* (Oxford, 1963), 3; and H. Wright, ed., *Francisco de Vitoria* (Washington, 1932), 31. For the suspicion that Suarez cannot properly be considered a social contract theorist, see F. Copleston, *A History of Philosophy*, 3, pt. 2 (New York, 1963): 167; for equivocation on Suarez, see J. Lares, *The Political Economy of Jean Mariana* (New York, 1928), 35, 38, 58; and, for explicit denials that Suarez had “an individualistic social contract theory,” see G. Lewy, *Constitutionalism and Statecraft during the Golden Age of Spain* (Geneva, 1960), 42; and H. Rommen, *Die Staatslehre des Franz Suarez, S.J.* (Gladbach, 1926), 112. For the assertion that Salamoni<sup>us</sup> advanced a social contract theory, see Atger, *Essai sur l'histoire des doctrines du contrat social*, 105; d’Addio, *L’idea del contratto sociale*; E. Barker, “A Huguenot Theory of Politics: The ‘Vindiciae contra tyrannos,’” *Proceedings of the Huguenot Society of London*, 14 (1930), 9; and J. W. Gough, “Review of M. d’Addio’s Works on Salamoni<sup>us</sup>,” *English Historical Review*, 72 (1957): 503, and *The Social Contract*, 47–48; and, for the claim that he did not, see Gierke, *The Development of Political Theory*, 102, as well as the works that present social contract theory as a seventeenth-century invention. For the argument that Bèze presented a social contract theory, see Laski, ed., *Vindiciae contra tyrannos*, 26; and, for the argument that he did not, since only Salamoni<sup>us</sup> and Mariana presented social contract theories in the sixteenth century, see Atger, *Essai sur l'histoire des doctrines du contrat social*, 105. For the assertion that Mariana had an explicit, though not “full-fledged” social contract theory, see Lewy, *Constitutionalism and Statecraft during the Golden Age of Spain*, 44, 46–47. For works that acknowledge Richard Hooker’s title as a social contract theorist, see Gough, *The Social Contract*, 72–75; William Haller, ed., *Tracts on Liberty in the Puritan Revolution, 1638–1647*, 1 (New York, 1934): 2; Clark, *The Seventeenth Century*, 217; G. Michaelis, *Richard Hooker als politischer Denker*, Historische Studien, no. 225 (Berlin, 1933); and F. J. Shirley, *Richard Hooker and Contemporary Political Ideas* (London, 1949); and, for works that do not, see J. W. Allen, *A History of Political Thought in the Sixteenth Century* (rev. ed., London, 1960), 194; and A. P. d’Entrèves, “Hooker e Locke: Un contributo alla storia del contratto sociale,” in Hans Kelsen et al., eds., *Studi filosofico-giuridici, dedicati a Giorgio del Vecchio nel XXV anno di insegnamento, 1904–1929* (Modena, 1931), 228–50, and *The Medieval Contribution to Political Thought* (Oxford, 1939), chaps. 5, 6. For a good survey of these disputes in terms of developments in Hooker studies, see W. D. J. Cargill Thompson, “The Philosopher of the ‘Politic Society’: Richard Hooker as a Political Thinker,” in W.

Many of these differences in judgment may be attributed, at least in part, to disputes about what constitute the “proper ingredients,” as it were, of a “genuine” contract theory. Common sense seems to dictate that we ascertain the elements of a theory before attempting to write its history. This is part of a set of organizing ideas that contributors to the literature have tended to share, for all of their disagreements on points of substance. It stems directly from the pioneering work of Gierke, to which the writings of J. W. Gough and Ernest Barker have added nothing of weight on matters of method and procedure. Gierke himself stands in the way of a more satisfactory history of contract theory. Many of his specific judgments have been disputed, but the main outline of the story he told and, above all, the principal organizing ideas he adopted have claimed

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Speed Hill, ed., *Studies in Richard Hooker* (London, 1972), esp. 6–12, 39–50. For the claim that Jean Bodin subscribed to a contractualist theory of sorts, see R. W. K. Hinton, “Bodin and the Retreat into Legalism,” in Denzer, *Jean Bodin*, 303–14; for an argument that the notion of a “State-Contract” was “held by Bodin as a self-evident postulate,” see Gierke, *The Development of Political Theory*, 117, n. 17; and, for the position that contractualist talk can be found in Bodin but that Bodin did “not have a contractual theory” and that the substitute for it is a highly precise empirical consideration of history, see J. H. Franklin, “Jean Bodin and the End of Medieval Constitutionalism,” in Denzer, *Jean Bodin*, 151–66. For a denial that Bodin’s thought contains any contractualist elements, see Plamenatz, *Man and Society*, 98. Hotman’s argument, so commonly viewed in contradistinction to the rationalist-contractualist views of the contemporary *Vindiciae contra tyrannos* and *De jure magistratum*, has nonetheless been interpreted as a contractualist treatise; see, for example, Murray, *The Political Consequences of the Reformation*, 188–89; and B. Reynolds, *Proponents of Limited Monarchy in Sixteenth-Century France: Francis Hotman and Jean Bodin* (New York, 1931), chaps. 2, 3. Even Althusius, so central to Gierke’s thesis as well as to Cassirer’s, has been denied the character of a contract theorist; see Becker and Barnes, *Social Thought from Lore to Science*, 383–86. C. J. Friedrich has stressed that Althusius’s writings are more appropriately interpreted as outlining a corporatist, rather than a contractualist, theory of the state; Friedrich, *Politica methodice digesta of Johannes Althusius* (Cambridge, 1932), Introduction. For an appreciation of the confusions and disagreements concerning whether other writers in the early modern period might properly be considered contract theorists, social contract theorists, or theorists of a noncontractualist sort, see A. S. P. Woodhouse, ed., *Puritanism & Liberty* (rev. ed., London, 1974), Introduction 71–78; Christopher Hill, *Puritanism and Revolution: Studies in Interpretation of the English Revolution of the 17th Century* (rev. ed., London, 1968), 270; William Haller, *Liberty and Reformation in the Puritan Revolution* (rev. ed., New York, 1963), 353–56, and *The Rise of Puritanism: or, The Way to the New Jerusalem as Set Forth in Pulpit and Press from Thomas Cartwright to John Lilburne and John Milton, 1570–1643* (1938; New York, 1957), 368–77; and Gough, *The Social Contract*, chap. 7. For the classic statement of doubt as to whether Hobbes, Locke, and Rousseau “properly” belong to the tradition of social contract speculation, see Gierke, *The Development of Political Theory*. Gierke characterized Hobbes as an opponent of the social contract “in the strict sense,” portrayed Rousseau as attacking the *Herrschaftsvertrag* which had become an integral part of social contract theory, and described Rousseau and Sidney as relegating the contract of rulership to secondary status (or dropping it altogether), while “the idea of the social contract . . . prepared itself for that exclusive dominion which Rousseau claimed for it”; *ibid.*, 103, 97–98, 109–10, 104–05. Plamenatz has suggested that Hobbes’s “initial assumptions make the social compact redundant”; *Man and Society*, 213; and similar arguments can be found with respect to Locke’s assumptions about the “sociable” state of nature and Rousseau’s insistence on the “General Will.” And the literature also abounds in all manner of controversies over issues such as whether contracts and states of nature were ever meant to refer to historical events or periods. For the idea that they were, see, for example, A. Elkan, *Die Publizistik der Bartholomäusnacht*, Heidelberger Abhandlungen zur mittleren und neueren Geschichte, no. 9 (Heidelberg, 1905), 29–30; and M. P. Thompson, “A Note on ‘Reason’ and ‘History’ in Late Seventeenth-Century Political Thought,” *Political Theory*, 4 (1976): 491–504. For the idea that they rarely or never were, see, for example, Cassirer, *The Myth of the State*, 217; Clark, *The Seventeenth Century*, 216; and Arendt, *On Revolution*, 10. Similarly, controversy seems rife as to whether social contract theory was ever really concerned with the origin of society or whether it was just concerned to portray the nature of society. For the idea that it was concerned solely with the nature and not the origin of society, see W. Kendall, “Social Contract,” in D. Sills, ed., *International Encyclopedia of the Social Sciences* (New York, 1968); and, for the idea that it was concerned with both, see Peter Laslett, “Social Contract,” in P. Edwards, ed., *Encyclopedia of Philosophy* (London, 1967). Again, disagreement is evident in interpretations of the practical political conclusion drawn by past writers of the “consequences” of adopting contractualist theories in political thought. For the idea that contractualism was always associated with individualism, liberalism, and constitutionalism, see Laslett, “Social Contract”; and Donald W. Hanson, *From Kingdom to Commonwealth: The Development of Civic Consciousness in English Political Thought* (Cambridge, Mass., 1970); and, for other works that share this view,

countless adherents.<sup>5</sup> Gierke contended that there is a model theory, that the parts of the model are logically articulated rather than contingently associated, and that the historian's task is to assess past contractarian writings according to the "juristic sharpness" with which the model appeared in them. And Gierke's work is also the source of that abiding belief that a proper contract argument should contain two components, *Gesellschaftsvertrag* and *Herrschaftsvertrag*.

The centrality of these notions to Gierke's history is explicit from the outset. He found his model theory in the writings of Althusius, a choice that has had few followers. But the criteria Gierke used for selecting Althusius have been repeated and reworked in many monographs and general histories. Thus, according to Gierke, Althusius was the first to raise

... the idea of contract to the level of a theory, in so far as he was the first to construct in a logical way a scientific system of general politics on the assumption of definite original contracts. . . .

Althusius takes as the basis of his theory the division of the processes of will assumed to institute the State into two parts, the Social Contract and the Contract of Rulership. This division was commonly accepted until the end of the eighteenth century and was first attacked by Hobbes. The distinction of these two contracts was stated much earlier, but never with such juristic sharpness. In any case each of these may be separately studied in its genesis and history. That which is logically posterior is taken first, simply because it is historically anterior.<sup>6</sup>

The reversal of logic and history presented no problem for Gierke. Given his broadly Hegelian assumption that history reveals the progress of Reason, he may well have expected that the logically complete contract theory would in time follow a number of logically incomplete sketches. His Hegelian pre-

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though with different assessments of the value of the association, see Gough, *The Social Contract*; Ernest Barker, ed., *Social Contract: Essays by Locke, Hume, and Rousseau* (London, 1947), Introduction; and Gierke, *The Development of Political Theory*. Gierke, however, insisted that the doctrine was "variously construed and made to serve such different political tendencies"; *The Development of Political Theory*, 105. Sabine made a similar point; *A History of Political Theory*, 367. The problems are such—in any attempt to associate contractualism exclusively with liberalism, rights of resistance, minority rights, or liberal constitutionalism when considering Hobbes, Spinoza, and Rousseau—that they are sufficient to cast doubt on the usefulness of such general associations. Many of the specific issues here may be traced to very broad differences of opinion as to whether the mere reference by an author to the term "contract" or one of its supposed synonyms—*foedus*, pact, *pactum*, compact, covenant, bargain, stipulation, alliance, consent, trust, and the like—is sufficient evidence that a contract theory has been posited or drawn upon or whether only a systematically worked out set of ideas with contract occupying a central place is necessary before a writer is admitted to the ranks of "contractualist thinkers." The first seems to be implicit in the work of Gierke and Gough, the second in the much more selective view of P. Riley; see Gierke, *The Development of Political Theory*; Gough, *The Social Contract*; and Riley, "How Coherent Is the Social Contract Tradition?" *Journal of the History of Ideas*, 4 (1973): 543–62. For a brief discussion of some of these issues, see G. del Vecchio, "Über die verschiedenen Bedeutungen der Lehre vom Gesellschaftsvertrag," in U. Klug, ed., *Philosophie und Recht* (Wiesbaden, 1960), 20–27.

<sup>5</sup> Gough, *The Social Contract*; and Barker, *Social Contract*, Introduction. Gierke is the most frequently cited author in Gough's work. Barker was Gierke's translator in 1934, and he referred his reader to Gierke's account of natural law theory since "the proper understanding of Locke and Rousseau demands a knowledge of the theory"; *Social Contract*, xii n. 1. But the assumption that there is a "genuine" contract theory has come under considerable attack in recent years, especially in the work of W. H. Greenleaf, J. G. A. Pocock, and Quentin Skinner.

<sup>6</sup> Gierke, *The Development of Political Theory*, 91.

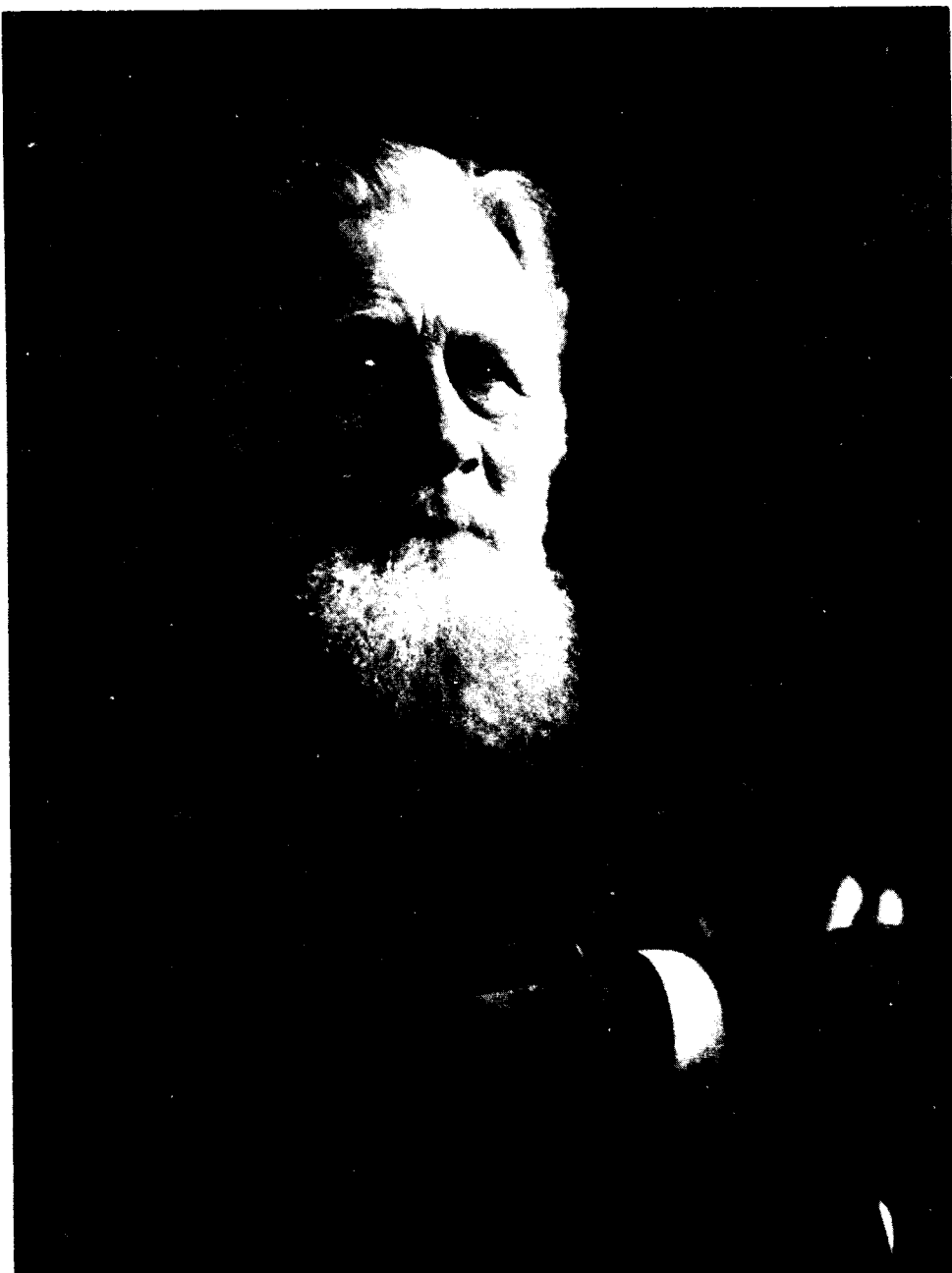
supposition would also have lent credence to the converse assumption that it is appropriate to view earlier or incomplete expressions of contract theory as if they were attempts to arrive at the later, logically complete theory, although Gierke himself was too sensitive to the uniqueness and particularity of his evidence always to carry through this recipe for anachronism. Yet, even among historians who have rejected the philosophical ideas that provide whatever ultimate coherence Gierke's story of contractualism might have, that story has been extraordinarily resilient, a resilience that can be accounted for by the continued acceptance of Gierke's organizing ideas despite the rejection of their philosophical basis.

GIERKE'S HISTORY of the supposedly logically posterior *Herrschaftsvertrag* established what has become a very familiar tale. A contract of rulership, he argued, was "accepted in the Middle Ages as almost beyond dispute." Its "earliest traces" were to be found in the positions of Manegold of Lautenbach and a number of cardinals during the Investiture controversies. Religious ideas, odd events and practices in some states of the Holy Roman Empire, legal ideas associated with the Roman *lex regia*—all fostered its "growth" in the Middle Ages. In the sixteenth century, Protestant and Catholic *monarchomachi* took the medieval idea of a contract of rulership as "the basis of their whole system" and so did numerous clerical writers like Diego de Covarruvias, Fernando Vázquez, and Francisco Suarez. Even Jean Bodin believed it a "self-evident postulate." By the seventeenth century, especially with the spread of modern individualism, the rulership contract had become established dogma with very few opponents; only its precise meaning and terms remained in dispute.

In the period after Althusius and Hobbes, according to Gierke, those who used the rulership contract found themselves increasingly concerned with a social contract as well. The notion of *Gesellschaftsverträge* supposedly came to the fore when the question of the origin of government was so widely settled by a contractualist answer that the prior question (how did the community that contracts with the ruler come into existence?) demanded an answer. The contract of society, in Gierke's account, gradually replaced immediate divine institution or the natural consequences of a divinely created human nature as the preferred answer to this supposedly logically prior question. To be sure, the idea of a social contract, like that of a rulership contract, had already been suggested in the Middle Ages, but it had "remained without further development." Even in the sixteenth century the idea was "but partially matured." Only with Johannes Althusius did it receive an appropriate and proper statement.

Althusius's system explained the state purely in terms of individual human willing and thus broke completely with the Christian and Aristotelian traditions. But, in taking that theory as the first "genuine theory of the Social Contract" and in interpreting Althusius as an individualist, Gierke found himself in difficulties. His history was written to culminate in Althusius, as the vocabulary





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of growth, development, and maturity bears witness. But Gierke had to admit that the Althusian theory was a historical anticlimax; it appeared only to be ignored. Grotius “failed” to rid himself of Aristotelian conceptions, and Hobbes, in failing to talk of a government contract and in not drawing constitutional conclusions similar to those of Althusius, proved himself to be an opponent of “the idea of a ‘social contract’ in the strict sense and [of] all the consequences drawn from it.” Clearly, a history of social contract that began and ended with Althusius would have been eccentric.<sup>7</sup> But Gierke was spared any embarrassment by detecting in the work of Samuel Pufendorf a social contract theory “after the manner of Althusius” that became “dominant” until Rousseau rejected the rulership contract. Pufendorf’s reviving role and Rousseau’s deathblow, however, only serve to underline the curiousness of Gierke’s judgment that the “culmination of the idea of social contract together with its abstract individualism was reached” in Johann Fichte’s early writings of the 1790s.

By thus organizing his history around a model contract theory that he believed was embodied in the writings of Althusius, Pufendorf, and Fichte, Gierke was obliged to cast aside the writings of Thomas Hobbes, Hugo Grotius, and Jean Jacques Rousseau—even to put them in the anticontractualist camp. Gierke was not attempting to understand his evidence in all of its particularity and complexity. He was simply reading into the evidence before him the contours of his preconstructed model and, thereby, created several problems. At one point, for example, Gierke argued that the clearest expression of the dual social contract–rulership contract theory in the sixteenth century was developed in the writings of Covarruvias, Francisco de Vitoria, Luis de Molina, and Suarez. This development was fundamentally individualist, opposed to theological and Aristotelian derivations of human society from God’s will or man’s natural sociability. Yet Gierke proceeded to assert that what characterized the social contract theory of the pre-Althusius, Spanish natural law school was the attempt “to divest the social contract theory of its original individualist character.” The authors cited to prove this point were precisely those—Suarez, Molina, Covarruvias, and Domingo de Soto—who had previously been named to show the rise of social contract theory in the sixteenth century. In other words, the same authors and writings were cited as evidence of both the rise of the theory and of attacks on its essential characteristics. And the contradiction here is heightened by Gierke’s insistence that the Spanish writers attacked the fundamental character of a theory that had not yet been fully developed, for, in Gierke’s account, there was no genuine social contract theory until Althusius formulated one in the early seventeenth century. By reading into the Spanish writers a concern to produce the sort of theory Gierke believed Althusius had conceived and in recognizing differences that he interpreted as attacks, Gierke cast the Spanish natural lawyers in a historically untenable role—that of criticizing a theory they

<sup>7</sup> It would have been doubly unfortunate in that Althusius, with his emphasis on “symbiotics” and the derivation of the state from the unit contractual society of the family, is certainly not the unequivocal “abstract individualist” that Gierke later argued was essential to the character of social contract theory. C. J. Friedrich commented on this point in his Preface to Carney, *The Politics of Johannes Althusius*, xi.

could not possibly have been concerned about and that of theorizing about a concept that they did not know.<sup>8</sup>

Such massive contradictions in a deservedly eminent scholar were hardly mere inadvertence. Rather, they arose from grave defects in Gierke's organizing ideas. Gierke's thought was permeated by the belief that there is, as it were, *a* or *the* theory of contract, a model to which actual writers more or less conformed. Whatever else such models do, they lead to a loss of sensitivity to context and to the specific. Very broadly, models act as levelers, reducing particular and complex evidence to predetermined contours. The results can be startling but never historically enlightening. One result of beliefs in such a model theory has been the encouragement of the idea—an idea that needs to be dislodged—that contract theory is a fixed star in the eternal firmament of political theory. A second result, equally as much a hindrance to a history of contractarianism, has been the belief that writers should be judged according to how closely they adhered to *the* contract theory in all of its logically necessary parts. What those writers endeavored to do, what questions they themselves asked, what evidence they introduced in support of their arguments and enquiries, what ideas they associated with appeals to contract—these considerations have been overshadowed or neglected. Until this approach is rejected, no viable history of contract is possible.<sup>9</sup> What is required is an approach that does not interpose a highly structured set of expectations and presuppositions between the historian and his evidence and yet permits discrimination between what should and what should not be included in a history of contractualism.

THE CONCEPT OF A "LANGUAGE" OR "VOCABULARY" of contract appears to meet these requirements.<sup>10</sup> It defines a determinate, limited subject-matter for a history of contractualism, permits the construction of a coherent story, and does not preclude—but, rather, encourages—a recognition of the diversity of uses to which contractual terminology was put. A history of contract construed according to this concept will be based on those political writings whose central contentions were made by recourse to one or another term from the family of contract-synonyms—a family composed of the terms of covenant, compact, contract, pact, paction, treaty, bargain, and agreement as well as their equivalents in Latin (*foedus*, *pactum* or *pactio*, and *contractus*) or other languages (*Vertrag*, *Bund*, *traicte*, *pacte*, and the like).<sup>11</sup> It will simply no longer do to interpret any

<sup>8</sup> See Gierke, *The Development of Political Theory*, 112 n. 1, 101, 126 n. 62.

<sup>9</sup> Recent monographs have begun to be much more critical of the variety of contexts in which contractualist talk took place; see, for example, Dennert, *Beza*, *Brutus*, *Hotman*, esp. xlv–li. But none have questioned the central organizing ideas of the Gierke account.

<sup>10</sup> We are here espousing, with reservations, the position of J. G. A. Pocock. See his *Politics, Language, and Time: Essays on Political Thought and History* (London, 1972). We have also found the theoretical articles of Quentin Skinner especially helpful. On Pocock, see Harro Höpfl, "John Pocock's New History of Political Thought," *European Studies Review*, 5 (1975): 193–206.

<sup>11</sup> The synonymy of these terms in the sixteenth and seventeenth centuries is confirmed by the translating habits of the time and also by contemporary dictionaries. See, for example, the *Oxford English Dictionary*; Cooper's *Thesaurus* (1565); T. Thomas, *Dictionarium Linguae Latinae et Anglicanae* (1587); J. Florio, *A Worlde of Wordes*

and every allusion to consent, consensus, or mutual obligations in any piece of political writing as a signal of the presence (at least *in nuce*) of a contract theory, still less of *the* theory of contract. Consent and covenant or contract are notions of quite diverse connotation; mutual and reciprocal obligations need not be contractual ones; and a history of contractualism is not, or not exclusively, a history of theories in any strict sense of that term, still less is it the history of *a* theory.

The decision to employ contractual terminology and the uses to which it has been put must be explained by reference to the purposes of those who have used it. The mere availability of contractual vocabulary cannot in itself explain either the decision or the uses. Once the decision to use contractual terminology has been made, however, it delimits a range of possible applications. Not every relationship can be construed contractually, and a writer who has decided to specify a contractual relationship finds his language making certain demands on him. For instance, the language intimates that he specify the parties (whether individuals or groups) to the covenant he has postulated, their precontractual condition (especially in respect of the formal liberty and equality requisite in contractors), the reciprocal undertakings entered into, the evidence that any covenant has been made (possible witnesses, authoritative records, or something of the sort), and so forth. But what may count as adequate answers to such questions depends on the purposes and ingenuity of the writer in question and on what he and his audience find plausible. What actually happened in any instance when contractual vocabulary was employed, what questions were being asked, and *how* contract was used to answer them must in each case be discovered by historical enquiry. The organizing idea of a language of contract has the virtue of not prejudging such matters.

Here only a rough sketch along these lines is possible. Such a history of contractualism in political thought may, however, be expected to address itself to three topics: (1) the availability of contractual terminology; (2) the circumstances of its first articulation into a political language; and (3) the patterns formed by the questions contract was used to answer. This last involves the constellations of concepts and preoccupations in which contract characteristically appeared, the vagaries of its fashions, and the like. It is a story that has a beginning—a language is more than a word—but no end. The reports of the death of contractualism so common in the past were not only premature but also misconceived: a developed political language is not something that passes away without a trace.

The availability of contract terminology is coeval with European political thinking. Despite their industry, Gierkean historians have dredged up only casual references in various political contexts to some treaty or agreement even though bargains and agreements of various sorts have been endemic in both intrapolity and interpolity relations. Indeed, the very term for a polity in the lan-

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(1598); R. Cawdrey, *A Table Alphabeticall of Hard Usual English Words* (1604); Cowell's *Interpreter* (1607); J. Bull-okar, *An English Expositor* (1616); H. Cockeram, *The English Dictionarie* (1626); T., *Glossographia* (1656); J. Kersey, *A New English Dictionary* (1702); and J. Grimm and W. Grimm, *Deutsches Wörterbuch* (mod. ed., Leipzig, 1956).

guage of scholarship was *societas*, which suggests *adsociato* or *consociato* and *socius*, alliances and allies, which in turn presuppose treaties, agreements, or covenants.<sup>12</sup> In interpolity relations, moreover, the association of ideas between making peace and making treaties was so close that a plausible, though apparently specious, etymology of the late sixteenth century derived *pactum* from *pax*.<sup>13</sup> And in the *Institutes* of Justinian, *societates* (private associations or corporations) are understood as constituted by *contractus*. Given this proximity to European habits of political speech, what perhaps calls for explanation is why the possibilities of contractual thought were not exploited more frequently or systematically.

More than merely casual references to covenants first figure in political writing as items in the Huguenot legitimization of resistance to established authority subsequent to the Saint Bartholomew's Day massacre of 1572.<sup>14</sup> Three productions of this period were remembered particularly in later years: François Hotman's *Franco-Gallia* (1573); Théodore de Bèze's *Du droit des magistrats* (1574; a Latin version, *De iure magistratus*, was published in 1576), and Junius Brutus's *Vindiciae contra tyrannos* (1579, but written about the same time as the other two). Of these only the last exploited the possibilities of contractual imagery to any considerable extent. Although Brutus's (Mornay's) purposes were highly specific and pragmatic, his formulations were sufficiently abstract to allow the work to be recycled for use in the Netherlands and (in a somewhat free translation) England in 1640 and again in 1689.<sup>15</sup> As a recognized minor classic of the genre, an investigation of the *Vindiciae contra tyrannos* may contribute something toward an understanding of the reception of contractual motifs into the mainstream of political thought.

The range of authorities that Mornay deployed provides a convenient list of sources for contractualism. In the first place, Mornay, like Beza and Hotman, contended that the relationship between ruler and ruled is one of *mutua obligatio*. That phrase, apart from its obvious congeniality to Huguenot political purposes, had been rendered definitive by Calvin. Like Martin Luther and Martin Bucer, Calvin had gone out of his way to insist that the duty to obey rulers was *not* conditional on either the title or the good conduct of rulers. Consequently, Calvin had no use for contractual talk in this context.<sup>16</sup> But the conceptual gap

<sup>12</sup> For *societas* as a commercial association generally, see for example, T. E. Holland, ed., *The Institutes of Justinian* (Oxford, 1881), esp. 3. 27. The term was, however, used both in Roman and medieval Latin for both private associations and for commonwealths, republics, kingdoms, and the like—the *societas perfecta* of the Scholastics, for example.

<sup>13</sup> Alberico Gentili, *De iure belli libri tres* (1598), trans. J. C. Rolfe and ed. C. Phillipson (Oxford, 1933), vol. 2, bk. 2, chap. 10, 176. Our thanks are due to K. J. P. Westney for this information.

<sup>14</sup> For a clear exposition of this point, see W. Näf, *Herrschaftsverträge und Lehre vom Herrschaftsvertrag*, Schweizer Beiträge zur Allgemeinen Geschichte, no. 17 (Bern, 1949). Impressions to the contrary rely on the occasional use of contractual terminology or, still more vaguely, on the presence of any sort of talk about consent, consensus, or agreement; see, for example, Gough, *The Social Contract*, 25, 28, 32 n. 5. No identifiable subject for a history of political thought can be created in this way, since it affords no viable principle for inclusion or exclusion. In the examples cited by Gough, the term *pactum* sometimes appears to mean no more than a promise; see *ibid.*, 30, 31, 37.

<sup>15</sup> These three treatises appear in an excellent abridgement and translation: Julian H. Franklin, *Constitutionalism and Resistance in the Sixteenth Century: Three Treatises by Hotman, Beza, & Mornay* (New York, 1969). For the *Vindiciae contra Tyrannos*, we are using the translation by H. J. Laski, who reproduced the translation of 1689: Junius Brutus [Mornay], *A Defence of Liberty against Tyrants*, ed. Laski (London, 1924).

<sup>16</sup> On Calvin, see M. Chenevière, *La Pensée politique de Calvin* (Geneva, 1937), 124–28; and J. Bohatec, *Calvins Lehre von Staat und Kirche* (Breslau, 1937), 62–69. But Bohatec's remark that "der Vertrag war ihm [i.e., Calvin]

between *mutua obligatio* and covenant is not insuperable (the latter is a species of the former), and it had already been bridged almost unconsciously by Buchanan and Bèze.<sup>17</sup> Furthermore, the political practice of poorly consolidated states—like the “German” Empire, Poland, and Scotland—and memories of such practice in better consolidated ones—like France, England, and Spain—suggested contractual notions.<sup>18</sup> Indeed, the relationship among the emperor and the electors, the most prominent *Landesherren*, and the Reichstag had *de facto* the character of an alliance or compact, as did the various religiopolitical “Leagues” of the period of the Reformation and the relationship between the kings and their “peers” in a feudal understanding. Finally, the most authoritative source of all was the scriptural conception of a contractual relationship between God and his chosen people, the Old and the New Covenant, as evangelicals translated it.

There was, then, no shortage of material on which to draw. But, whereas Beza had used all of these sources, and yet made covenant only one of a range of arguments tending in the direction of a relationship of *mutua* (and now conditional) *obligatio* between rulers and ruled and whereas Hotman hardly needed the notion at all, Mornay attempted to make covenant central to his account of political relations. More precisely, he used an idiosyncratic explanation of the scriptural covenant (*foedus sive pactum*; in French, *alliance*, *pacte*) as a model of right order, which, according to him, was in law and in fact recognized in all

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ein Denkmittel der organischen Beziehung zwischen dem Haupt und den Gliedern” is as wrong as its logic is curious; see *ibid.*, 73.

<sup>17</sup> G. Buchanan, *De jure regni apud Scotos* (1579–80), translated by D. H. MacNeill as *The Art and Science of Government amongst the Scots* (n.p., 1964), 65–67, 96: “There is . . . a mutual contract between king and people.”

<sup>18</sup> The term “contract” or one of its equivalents was frequently employed in the language of practical politics in these various states for both the activity of arriving at legal and political decisions (contracting) and the decisions and laws themselves (contracts). For the German Empire, see, for example, the Tübinger Vertrag (1514), in W. Näf, ed., *Herrschaftsverträge des Spätmittelalters*, Quellen zur neueren Geschichte, no. 17 (2d ed., Bern, 1975); and F. L. Carsten, *Princes and Parliaments in Germany: From the Fifteenth to the Eighteenth Century* (Oxford, 1959). For Castile and Aragon, see Ralph E. Giesey, *If Not, Not: The Oath of the Aragonese and the Legendary Laws of the Soborbe* (Princeton, 1963); J. H. Elliott, *The Revolt of the Catalans* (Cambridge, 1963), esp. chap. 1; and G. Griffiths, *Representative Government in Western Europe in the Sixteenth Century: Commentary and Documents for the Study of Comparative Constitutional History* (Oxford, 1968). It was quite common for the Cortes of Castile to have attached conditions to financial aid (*servicio*) to the king and to have regarded these as a contract; for example, in 1600 the Cortes at Madrid referred to “el contrato deste servicio”; see Griffiths, *Representative Government in Western Europe in the Sixteenth Century*, 7, 71. For France, see especially the activities of the Huguenot assemblies, which established themselves into associations through contracts (*contracter*); *ibid.*, 265–66; and L. Anquez, *Histoire des assemblées politiques des réformés de France, 1573–1622* (Paris, 1859). For Holland, see, for example, the renegotiations of the *Joyeuse Entrée* of Brabant, dating from 1356 and renewed with each new duke. Apparently without being aware of the associations of his language, Griffiths referred to the *Joyeuse Entrée* as a “constitutional contract between the ruler and his subjects”; *ibid.*, 302. Unfortunately, his translation of the sixteenth-century Dutch mistakenly inserts the term “contract” into the *Joyeuse Entrée*; he rendered “In welker dingen oirconden ende eeuwiger vesticheyt hebben wy onsen zegele hier aen doen hangen” as “Unto this contract, document, and permanent establishment we . . . have appended our seal”; *ibid.*, 348–49. The Dutch term *vergelijken* and the French *contracts* and *contrat* were, however, common in constitutional language as well as in the extraordinary *Contracten* (as, for example, in the Dutch version of the *Declaration of Independence* of July 26, 1581). See *ibid.*, 429, 431, 461–62, 456, 493, 494, 495, 499, 502, 511–12, 518. J. F. Maclean has suggested that Scottish constitutional relations and political practice (especially the custom of banding or bonding) were one of the sources of the idea of covenant; Maclean, “Samuel Rutherford: The Law and the King,” in G. L. Hunt, ed., *Calvinism and the Political Order* (Philadelphia, 1965), 69–72. For the suggested importance of political practice in Switzerland and Holland, see Friedrich’s Preface to Carney, *The Politics of Johannes Althusius*, ix. For Poland, see B. Connor and J. Savage, *The History of Poland* (London, 1698); and the anonymous *The Ancient and Present State of Poland* (London, 1697).



“well-constituted kingdoms.” A commonwealth is, by his account, to be understood as constituted by two covenants. One was between the king and the people as one party and God as the other; and, in an analogy (a very rare one) drawn from contract law, Mornay declared that the king and the people, or their authorized representatives, were coguarantors, or cosignatories, each responsible to God for the conduct of the other. The second covenant was between the king and the people or, rather, the people’s representatives: “The people are the stipulator, and the king the promisor. The people . . . asked whether the king would rule justly and according to the law. He then promised to do so. And the people . . . replied that they would faithfully obey, as long as his commands were just.”<sup>19</sup> The burden of the book is that, when kings become tyrants, resistance by “the people” is not just a right but a duty, both religious and civil, resulting from the covenant.

AFTER THIS BOLDLY CONTRACTUAL BEGINNING, it is striking to discover the range of things that Mornay did *not* explain contractually. He was, of course, obliged to substantiate his contention that “the people” could be a party to any covenant; this presumably was the reason for his repeated assertion that “the king cannot rule without a people, while a people can rule itself without a king.”<sup>20</sup> At the same time his (and, for that matter, Bèze’s) insistence on this point suggests one of the reasons why contractual imagery was so rarely employed. For the commonweal was almost universally understood as in some sense a *natural* unity; organic metaphors were generally thought indispensable to characterize it. Yet the relations between the parts of an organism cannot plausibly be construed as contractual. The head and members of a body (or body politic, an almost inescapable correspondence or similitude) are not joined by covenants; they are incapable of independent existence, and the head is not appointed to its function by the feet. In the same way, when commonwealths already in being were at issue, all possible parties to any putative covenant were already *parts* of the commonwealth, *defined* by their relationship to one another and to the whole of which they were parts. They thus were demonstrably not possessed of the independence and formal equality required of parties to a covenant. Mornay did not depart from accepted ideas in this, or in most other, respects. He was able to treat “the people” as capable of agency and of being a party to a covenant because he assumed all along a people already politically articulated and organized by subordination to lesser magistrates.<sup>21</sup> Only as “represented” and directed by such lesser magistrates did Mornay deem the people both capable of

<sup>19</sup> Mornay, *Vindiciae contra tyrannos*, in Laski, *A Defence of Liberty against Tyrants*, 174–75, and in Franklin, *Constitutionalism and Resistance*, 180–81.

<sup>20</sup> *Ibid.*, in Laski, *A Defence of Liberty against Tyrants*, 122, 125, and in Franklin, *Constitutionalism and Resistance*, 160. Also see Beza, *Du droits des magistrats*, in Franklin, *Constitutionalism and Resistance*, 104.

<sup>21</sup> In a momentous aside, Calvin had admitted that “popular magistrates”—that is, public officials individually inferior in status to kings—might be entitled, indeed obliged, by the law of the land to defend the people; Calvin, *Institution of the Christian Religion*, bk. 4, chap. 20, 31. The passage continued unchanged throughout all of the editions of the *Institution* from 1536 onward.

collective agency and entitled to act. Political initiatives on the part of private persons were as much anathema to Mornay as to any other man of his time sensitive to the dangerous potentialities of “the beast with many heads.” Nowhere did he attempt to explain contractually the nature of “the people” as a political collectivity.

Again, Mornay persistently spoke of one of the parties to the covenants as the king and not, as logic seems to require, the king-to-be. In fact, nowhere did he bother to distinguish between the derivation of the kingly *office* and the right to be an incumbent to that office. It is, of course, true that he attempted to establish a congruence between, on the one hand, what he (like Hotman) claimed to be the ancient custom of the French realm and “all well-constituted kingdoms” and, on the other, his theologicopolitical argumentation about covenants. And this forced congruence exacted a price. He was, for instance, obliged to find some evidence in actual practice for the covenantal relations he postulated. He found what he sought in coronation oaths and popular acclamations. Such acts not only suggested the conditionality of kingly authority, they also had the formality requisite for a covenant, and they were the practice even in hereditary monarchies,<sup>22</sup> a form of government that Mornay (again like Hotman) was obliged to recognize as not only legitimate but also the norm of the day.

Furthermore, Mornay speculated on occasion about “what kind of rulers [the people] were most likely to have sought” when they first established kings,<sup>23</sup> and he felt confident that “men would not have surrendered their natural liberty . . . but for some special and great advantage.”<sup>24</sup> Such talk of covenant invites consideration of the precontractual condition. Mornay’s assumptions about the ancientness of the French constitution suggested locating that precontractual condition in remote antiquity, and rights enjoyed that are not civil or legal must be natural. Among these Mornay specifically listed self-preservation and property. But all of this remained peripheral and merely suggestive in the *Vindiciae contra tyrannos*. Mornay was, in fact, quite uninterested in explaining the origins of the kingly office or in the derivation of titles to it (he casually allowed that a usurper might gain a title by subsequent good conduct),<sup>25</sup> and his references to the distant past are, for the most part, no more than an attempt to capitalize on the contemporary assumption that in matters of law and institutions, what is older is, *ceteris paribus*, better.

Just as Mornay was generally content to assume the existence and legitimacy of the kingly office, so he made no attempt whatever to explain the derivation of

<sup>22</sup> The interpretation of coronation oaths as contracts or as evidence of contracts was very common. See *Le Réveille-Matin des François* (1574), summarized in François Hotman, *Francogallia*, ed. R. E. Giesey and J. H. M. Salmon (Cambridge, 1972), 74; Buchanan, *De iure regni apud Scotus*, LXXXV, LXXXVI; R. Parsons, *A Conference about the Next Succession to the Crown of England* (London, 1593); James I, *The Trew Law of Free Monarchies: or, The Reciprocal and Mutuall Dutie betwixt a Free King and His Naturall Subjects*, in C. H. McIlwain, ed., *The Political Works of James I: Reprinted from the Edition of 1616* (New York, 1956), 68; J. Tyrrell, *Bibliotheca Politica* (London, 1727), 739; W. Atwood, *The Fundamental Constitution of the English Government* (London, 1690), 32; E. Cooke, *Argumentum Anti-Normannicum* (London, 1682), xvi; and P. Allix, *An Examination of the Scruples of Those Who Refuse to Take the Oath of Allegiance* (1689), in *State Tracts*, 1 (London, 1705): 302.

<sup>23</sup> Mornay, *Vindiciae contra tyrannos*, in Laski, *A Defence of Liberty against Tyrants*, 139–40, and in Franklin, *Constitutionalism and Resistance*, 168, 171.

<sup>24</sup> *Ibid.*, in Laski, *A Defence of Liberty against Tyrants*, 134–44.

<sup>25</sup> *Ibid.*, in Laski, *A Defence of Liberty against Tyrants*, 117.

those lesser magisterial offices so crucial to his case. The reason for his casualness is easy to see: the king and the lesser magistrates that Mornay had in mind were an actual king and actual lesser magistrates. Both were parties organized and armed, both were powers in the land; and Mornay's point was that the only way to reintegrate them into one commonwealth was by a treaty of peace, on conditions acceptable to both. Those conditions, Mornay thought, were embodied in the "fundamental laws" of the French realm.<sup>26</sup> He did not explain either the content or the authoritativeness of these fundamental laws by reference to his covenants—in fact, he did not attempt any such explanation at all. The fundamental laws were simply a principal datum for him as they were for his contemporaries. Nor did he explain how the natural rights and liberty he had mentioned should be deemed to be related to these laws. He was simply not concerned with the natural rights of precivil individuals but only with what he optimistically took to be the positive legal rights of Calvinist congregations, lesser magistrates, and the political corporations of the French realm. These were fundamental rights and laws that preceded any particular king or group of lesser magistrates; from them individuals derived their offices and corporations their rights; kings swore to uphold them; and they were what entitled or rather obliged lesser magistrates to resist "tyrants"—that is, incumbents of the royal office unwilling to rule in accordance with these fundamental laws. And, *pace* Gierke, there was nothing remotely individualistic about any of this.

Thus, Huguenots used talk about covenants as one of a number of possible ways to support their contention that a relationship of mutual, but also conditional, obligation existed between rulers and ruled. Such talk aided them in the difficult task (difficult especially for evangelicals) of making resistance to supposed tyrants legitimate. Covenant, however, did not seem to Huguenots to explain the nature of the political community. Assumptions about the existence of an ancient constitution and about the naturalness of hierarchy and sociality prevented the articulation of any such evidently conventional, artificial view of the character of civil society. At best, the concept of a covenant was useful, because it married reasonably well with the belief in a reciprocity of rights and duties that not even the most determined advocate of monarchical absolutism was disposed to deny.<sup>27</sup> The division of France into warring parties made a treaty (*pacte*) with mutual guarantees the obvious remedy: *pax* by *pactus*.

WITH THE SUBSEQUENT ADOPTION OF CONTRACTUAL TALK by the Catholic *Ligue*, when the tables were turned, and by the Netherlanders, who indeed utilized these French Huguenot treatises,<sup>28</sup> covenant may be said to have emerged as a

<sup>26</sup> The term *lois fondamentales* first made its appearance in French literature during this period. See A. Lemaire, *Les lois fondamentales de la monarchie française d'après les théoriciens de l'Ancien Régime* (Paris, 1907); and J. W. Gough, *Fundamental Law in English Constitutional History* (Oxford, 1955), 50-51.

<sup>27</sup> Bodin, for example, was quite unequivocal on the point. His *Six Livres de la République* (1576) are contemporary with the other works cited here.

<sup>28</sup> See E. H. Kossmann, "The Development of Dutch Political Theory," in J. S. Bromley and E. H. Kossmann, eds., *Britain and the Netherlands*, 1 (London, 1960): 91-110; E. H. Kossmann and A. F. Mellink, eds.,

*topos*, a part of the conceptual arsenal at hand whenever the relationship between rulers and ruled was up for discussion. But it would be facile to conclude that the *topos* was bound to make the transition from the polemical *livre de circonstance* to the theoretical treatise, for it was in a sense compromised by the circumstances of its first employment. And, indeed, covenant long retained its association with the experience of religiopolitical belligerence that strongly commended absolute monarchy to many in all but a few sheltered places during the seventeenth century.<sup>29</sup>

The difficulties of this transition have been disguised by the acceptance of the Gierkean interpretation of those writers, generally scholastics, who were less concerned to subordinate the flow of thought to the exigencies of some cause or current opinion and who were widely admired by Catholics and evangelicals alike. What these writers in fact asserted was the foundation of political authority in *consent*, not covenant. The finest English writer in this genre, Richard Hooker, serves to illustrate the kind of account of political authority that they offered.<sup>30</sup> In a few elegant phrases Hooker explained how a commonwealth or politic society might be at once natural and yet the product of human artifice:

To supply those defects and imperfections which are in us living singly and solely by ourselves, we are naturally induced to seek communion and fellowship with others. This was the cause of men's uniting themselves at first in politic societies, which Societies could not be without Government, nor Government without distinct kind of law. . . . Two foundations there are which bear up public societies: the one, a natural inclination [to social life]; the other, an order expressly or secretly agreed upon touching the manner of their union in living together. The latter is that which we call the Law of the Commonwealth, the very soul of a commonweal.<sup>31</sup>

Equally succinctly he stated the efficient and formal causes of government: "To take away all . . . mutual grievance, injuries and wrongs, there was no way but by growing unto composition and agreement amongst [men in those times when there were no civil societies] by ordaining some kind of government public." Common consent to such government is necessary because, without it, "there were no reason that one man should take upon him to be lord and judge over another."<sup>32</sup> (The past tense, "first beginnings" formulation here is somewhat misleading. The reference is not to prehistory but to a possible and "natural" establishment of a *societas perfecta*, as Aristotle and Thomas Aquinas had envis-

*Texts Concerning the Revolt of the Netherlands* (Cambridge, 1974), esp. 165–66, 174–75, 185, 197, 199, 216, 217, 228, 268–70; and J. H. Salmon, *The French Religious Wars in English Political Thought* (Oxford, 1959).

<sup>29</sup> Defenders of royal absolutism liked to make great play with the genesis of such talk out of an unholy alliance of Puritans and Papists. See, for example, Robert Filmer, *Patriarcha: or, The Natural Power of Kings As-asserted*, ed. Peter Laslett (Oxford, 1949), esp. 53–55.

<sup>30</sup> For relevant writings in this group, see Domingo de Soto, *Libri X de justitia et jure* (1556) (a *bon mot* of the time had it that "qui scit Sotum scit totum"); Robert Bellarmine, *De laicis ac potissimum de magistratu politico* (bk. 3 of *De ecclesia militante* [1586–93]); L. Molina, *De justitia et jure opera omnia* (1593–1618); J. Mariana, *De rege et regis institutione, libri III* (1598); and F. Suarez, *De legibus ac Deo legislatore* (1612). The precise relationship between the works of these writers and those of Richard Hooker has never been satisfactorily established.

<sup>31</sup> Hooker, *The Laws of Ecclesiastical Polity*, ed. C. Morris, 1 (London, 1965): 188.

<sup>32</sup> *Ibid.*, 190.

aged it.) Plainly, accounts of this sort had little use for covenant, and those who, like Grotius, stood in this tradition managed perfectly well without it and its attendant difficulties.<sup>33</sup>

Apparently, the German Calvinist Johannes Althusius made the first attempt to bring contractual language explicitly derived from Huguenot and *Ligue* sources and the scholastics' conceptions of civil society together into a self-consciously scholarly political theory. For this reason Althusius has an important place in the history of political contractarianism and not, as Gierke thought, because he provided a model of such an account. In the *Politica methodice digesta* (first published in 1603, often re-edited and reprinted) Althusius insisted on interpreting *all* significant relationships within the *societas perfecta* as contractual. The relationships were not simply consensual—that was a scholastic commonplace—but were founded on covenants (*pacta*, *foedera*, or *conventus*). Althusius thus interpreted civil society as an association of lesser associations. The elementary association was the family, which was initiated by a *pactum* and involved a *mutua obligatio*. After the family came private associations (*collegia*, *societates*, and the like), all joined together by *pacta* and laws. The association of private associations was the city; an association of cities was a province; and an association of provinces, cities, and estates was the “universal public association.” Sovereignty in the public association was not vested, as Bodin supposed, in some absolute monarch but in the public association—that is, in the people or their representatives. Indeed, for Althusius the sovereignty was not absolute at all, since it was limited by both natural and divine law. The supreme magistrate himself attained his power from covenants entered into by and with the whole people. Althusius went further: “No realm or commonwealth has ever been founded or instituted except by contract entered into one with the other, by covenants agreed upon between subjects and their prince-to-be, and by established mutual obligations that both should religiously observe.”<sup>34</sup>

The Huguenot ambivalence about whether covenant referred to the institution of the kingly office or only to the installation of determinate individuals in that office reappears in *Politica methodice digesta*, although Althusius apparently meant both. But it did not greatly matter, for, like his Huguenot predecessors, Althusius presupposed a “fundamental law of the kingdom.” But he now interpreted this contractually as well. “This law serves as the foundation, so to speak, of the realm and is sustained by common consent and approval,” and it is “nothing other than certain covenants [*pacta*] by which many cities and prov-

<sup>33</sup> This is not to deny, of course, that Grotius was sometimes prepared to discuss the origin of the state in terms of contract. Indeed, at one point he argued for the inappropriateness of natural body-body politic analogies since the body politic was of a different kind, “since it was formed from a voluntary compact”; Grotius, *De jure belli ac pacis libri tres*, ed. J. B. Scott (Oxford, 1925), bk. 2, chap. 6, sec. 4, 261. But, when discussing the character of contracts, Grotius never considered the state itself as contractual; see *ibid.*, chap. 12. When considering the origin of civil society, he merely talked of men joining themselves together “of their own free will”; *ibid.*, bk. 1, chap. 4, sec. 7, 3. When discussing the origin of legitimate power, he pointed to acquiring “a right through long possession or contract”; *ibid.*, chap. 4, sec. 15, 1. And, when discussing the right of a state over its subjects, he was concerned with that “most perfect society” of an “association in which many fathers of families unite into a single people and state” that “gives the greatest right to the corporate body over its members”; *ibid.*, bk. 2, chap. 5, sec. 23.

<sup>34</sup> Althusius, *The Politics*, ed. Carney, 117. Althusius, unlike Mornay, talked of kings-to-be.



inces come together and agree to establish and defend one and the same commonwealth. . . . When common consent is withdrawn from these covenants and stipulations, the commonwealth ceases to exist.”<sup>35</sup>

Apart from illustrating the magical ring<sup>36</sup> that the word “covenant” had in Calvinist ears, Althusius’s theory is instructive for two reasons. In the first place, Althusius plainly could no longer characterize a legitimate association or account for legitimate authority without recourse to ideas of will, artifice, and consent—despite his inordinate fondness for body–body politic and head–king similitudes and despite his elaborately biological vocabulary (he described civil society as *symbiosis*, citizens as *symbiotes*), all of which imply that both society and hierarchy are natural rather than artificial. And just as he was unable to accept civil society as simply a natural datum—he was after all a citizen of the Empire—so he could also no longer interpret fundamental law as given and immemorial (perhaps as a result of habits of thought begotten of Roman law, which he frequently cited, and of the persuasiveness of Bodin’s concept of sovereignty, which was ever before his mind, even while he was trying to wrest sovereignty from kings and redistribute it to “the people”). The same rejection of the belief that relations of authority and obedience are in any sense natural is discernible in scholastic writing, as we shall have cause to note in a moment. The notion of covenant stood to gain from this tendency, for it afforded a ready and easy way to establish obligations and rights that were not in any obvious sense “natural.”

Althusius’s theory is instructive for a second reason. The bearers of rights in his conception are still unmistakably associations, groups, and corporations of various sorts, not individuals. And (again *pace* Gierke) for all of the changes he rang on the concept of covenant, one covenant he did not contemplate—namely, a covenant between *individuals* equipped with natural rights. There is, of course, no reason why he should have thought of such a covenant, although (as we know with the benefit of hindsight) this is the covenant that Hobbes made into a durable form of political argument. To imagine a society decomposed into, or a presocial condition made up of, individuals and loose groupings posed no difficulty. A condition of solitariness before civil society, perhaps succeeding the “Golden Age,” was a humanist commonplace. The problem was that such a condition was often interpreted as a *bellum omnium contra omnes*, an image that Hobbes by no means invented.<sup>37</sup> An obvious inference from such a premise was

<sup>35</sup> *Ibid.*, 123.

<sup>36</sup> The phrase is that of Georg Oestreich, “Die Idee des religiösen Bundes,” 140.

<sup>37</sup> It had a place, though not *eo nomine*, in Pope Pius II’s work, where the following epic of humanist syncretism appears: “For when men, after the expulsion of the progenitors of the human race from paradise, wandered about in fields and forests after the manner of wild beasts, and lived like them, they perceived (for God has created them beings capable of reason) that social union is the most useful and necessary thing for man”; A. Sylvius Piccolomini [Pius II], *De ortu et authoritate imperii Romani* (1445), as given in Voigt, *Der Herrschaftsvertrag*, 81. Mariana presented a similar image: “Men at first wandered solitary [*solvagi*] like wild animals without laws or governors [*rectores*].” He then proceeded to an account of the concomitance of discoveries of means to a better life and the deterioration of morals, until “no place was safe for innocence” and those oppressed by the mighty began “mutuo se cum aliis societatis feodere constringere”; *De rege et regis institutione*, bk. 1. This is the origin of the “royal majesty,” and Mariana also remarked that in those days there were “multitudes of kings,” a remark that Filmer picked up in the interest of patriarchalism.



the imperative need for a strong, coercive authority, which is hardly what most early proponents of contractualism had in mind.

ALTHOUGH IT IS NOT POSSIBLE within the present compass to explain how it came to seem plausible to conceive of civil society as a conditional and covenanted association of individuals enjoying "natural rights," certain observations are in order. The rejection of the belief that relations of authority and obedience are in any obvious sense natural was already near at hand when the scholastics distinguished between the natural rightness of governance and the conventional legitimization of specific governors and forms of government and equally when they distinguished sharply between paternal (that is, natural) and political authority. The latter distinction became increasingly accepted, as Robert Filmer's attack on it shows.<sup>38</sup> Clearly, contractual notions played little part in the process. Rather, the sense of individual entitlement to autonomy, however generated, was articulated by exploiting the potentialities of the scholastic concepts of "natural law," "natural rights," and "natural liberty" (Filmer's particular bugbear).<sup>39</sup> Hobbes made this proximate source for the terminology of political individualism plain: "They that speak of this subject use to confound *ius* and *lex*, right and law; yet they ought to be distinguished."<sup>40</sup> Those guilty of such confounding were obviously late scholastic authors, and in their context the distinction was of little consequence: one man's *ius* is another man's *lex*.

Furthermore, the need to assert individual (as opposed to corporate) rights against both civil society and governors was evident only in one milieu. The sectarian Calvinists or congregationalists and sects tending to Anabaptism needed to assert a right to assemble and worship in churches of their own choosing and making.<sup>41</sup> Since no state conceded such a right, it could not be presented as a civil right; it had therefore to pass as a natural or divine right. Membership in such churches, moreover, required a conscious decision and a willingness to risk many hardships. Since membership was obviously voluntary, obedience could be nothing but conditional. Not surprisingly, too, the extent of the rights possessed by individual members of such congregations with respect to each other and to such authorities within the church as the congregations were prepared to tolerate was unprecedented. Naturally, these congregations articulated the character of their unions as contractual, subject to the fundamental law of the Gospel.

<sup>38</sup> Filmer, *Patriarcha*, *passim*. Also see, for example, Suarez, *De legibus ac Deo legislatore*, given in Voight, *Der Herrschaftsvertrag*, 116.

<sup>39</sup> For probably the most sophisticated discussion of this topic, see R. Schur, *Individualismus und Absolutismus* (Berlin, 1963), *passim*.

<sup>40</sup> Hobbes, *Leviathan: or, The Matter, Forme, and Power of a Commonwealth Ecclesiasticall and Civill* (London, 1651), chap. 16.

<sup>41</sup> The following account draws on three works in particular: C. Burrage, *Early English Dissenters*, 2 vols. (Cambridge, 1912); H. Dexter, *Congregationalism* (New York, 1880; reprint ed., London, 1970); and W. Förster, *Thomas Hobbes und der Puritanismus* (Berlin, 1969).

That the separatist congregations readily drew analogies between their congregations and civil societies is curious, for the basis for such analogies seems nonexistent. Separatist congregations were voluntary, located in one place, small, composed entirely of adults, and existed within civil societies whose legitimacy they did not deny. In no way did they resemble civil societies. Yet by 1613 the English separatist Henry Jacob defined a “visible Church of Christ under the Gospel” as “a Spiritual Body Politike . . . ; the people also having power of free consent in their ordinary government.”<sup>42</sup> And in 1616 the same author answered the question, “How is a Visible Church constituted and gathered?” with a striking formulation:

By a free mutuall consent of Believers joyning and covenanting to live as members of a holy Society together in all religious and vertuous duties as Christ and his Apostles did institute and practise in the Gospell. By such free mutuall consent also all Civill perfect Corporations did first beginne.<sup>43</sup>

The Covenant of the “Pilgrim Fathers” of 1620 simply converted the church into a civil society: “Whe . . . do, by these presents, solemnly and mutually in the presence of God and of one another, covenant and combine ourselves together into a civil Body Politick, for our better Ordering and Preservation. . . .”<sup>44</sup> Sectarians, then, interpreted their churches as free and voluntary associations founded on covenants. And, by a process that has yet to be fully explained, they appear, having compared their congregations to bodies politic, to have gone on to interpret bodies politic as if they were sectarian congregations. In so doing, they attributed to citizens, as natural rights, the rights of moral autonomy and self-government that they had demanded for themselves as members of the congregation.<sup>45</sup>

To derive parallels between church and state government was, of course, not at all unusual. This argument was common in Catholic and royalist thought. What was unusual was the proclivity to push the analogy so far—almost, indeed, to the point of identity. The basis for such an identification was laid early in the practices of Huguenot assemblies and in debates among Calvinists that occurred during the first religious war in France in the 1560s.<sup>46</sup> As soon as the Huguenots had taken to arms in March of 1562, they set about establishing their own political assemblies. The Huguenot provinces came close to constituting separate states within France, and many pursued policies designed to make the province coterminous with the local church. The province of Langue-

<sup>42</sup> Jacob, *An Attestation of Many Learned, Godly, and Famous Divines* (London, 1613), as quoted in Förster, *Thomas Hobbes und der Puritanismus*, 96.

<sup>43</sup> Jacob, *A Confession and Protestation of Faith* (London, 1616), as quoted in Förster, *Thomas Hobbes und der Puritanismus*, 97.

<sup>44</sup> Jacob, *A Confession and Protestation of Faith*, as quoted in Förster, *Thomas Hobbes und der Puritanismus*, 107.

<sup>45</sup> Regarding “commonwealths by institution,” Hobbes, who was no separatist, spoke of the assembly that makes the covenant as a “congregation,” a term for the body politic that he used nowhere else.

<sup>46</sup> The following account draws on three works in particular: Griffiths, *Representative Government in Western Europe in the Sixteenth Century*; Anquez, *Histoire des assemblées politiques des réformés de France*; and R. M. Kingdon, “Calvinism and Democracy: Some Political Implications of Debates on French Reformed Church Government, 1562-72,” *AHR*, 69 (1963-64): 393-401.

doc illustrates this point. An “assemblée generale des estats dudict pais de Languedoc” met in November 1562. Its structure and proceedings followed the pattern of the province’s traditional estates, but there were a number of remarkable innovations. The members formed themselves into an association and elected their “chef, deffenseur et conservateur.” They then made a “contract” with him, setting down general guidelines for his rule, drew up a new constitution to be sworn to by the assembly’s delegates and by the ministers and inhabitants in the towns under their control, and finally passed legislation allowing for the expulsion of papists and the welcoming of Huguenot refugees from Catholic persecution.

The assembly did not, however, take the step of severing the province’s links with the French monarchy. The “contract” insisted that the civil “laws of the kingdom” be preserved “without making any changes or innovations in them, direct or indirect.” And the delegates announced their intention to collect the king’s revenues but only to hand them over and render account when hostilities ceased. By the time of the second civil war the religious policy in Languedoc had been of such effect that the political assembly of December 1569 could refer to itself as an “assemblée generale des dioceses et Églizes du bas pais de Languedoc.” The diocese was both a civil and an ecclesiastical district but the term “église” emphasized that the constituencies were composed of persons adhering to the Reformed churches. There was no confusion here of Church and state. The assembly was not a synod; rather, membership of a civil constituency was coterminous with church membership.

Similar ideas and practices were current in French Protestant political assemblies until the Peace of Montpellier in October 1622 forbade the Calvinists to conduct their own assemblies. Particularly important, however, is that these practices in Protestant provinces of France were accompanied by a lively debate within the French Reformed Church that turned precisely on just how acceptable it was to make analogies between Reformed Church government and civil government. During the 1560s Bèze emerged as the chief proponent of the limitations of the analogy while Morély stressed its entire appropriateness. The debate apparently lost much of its urgency after the Saint Bartholomew’s Day massacre. But it has been suggested that it continued in England, where Bèze’s argument was taken over by the Presbyterians and Morély’s by the Congregationalists.<sup>47</sup> Certainly, however, by the end of the sixteenth century the long debate within Calvinist ranks had established a firm precedent for transferring the conception of Reformed Church governance to the governance of the state.

The congregation, in other words, became for sectarians the paradigm of good order. Nowhere is this more apparent than in the writings of the Levellers, the familiarity of which renders a detailed investigation here unnecessary.<sup>48</sup> Suffice it to say that the Leveller leaders, with the exception of the elusive John

<sup>47</sup> Kingdon, “Calvinism and Democracy,” 399.

<sup>48</sup> See, for example, the accounts in the following texts and collections: D. M. Wolfe, ed., *Leveller Manifestoes of the Puritan Revolution* (London, 1967), Introduction; Woodhouse, *Puritanism and Liberty*, Introduction; G. E. Aylmer, *The Levellers in the English Revolution* (London, 1975); J. Frank, *The Levellers* (Cambridge, Mass., 1955); and C. B. Macpherson, *The Political Theory of Possessive Individualism* (Oxford, 1962), chap. 3.

Wildman, were all noted sectaries and tolerationists, who figured in Thomas Edwards's *Gangraena* (1646), the chamber of sectarian horrors of the Presbyterians, before the establishment of the Leveller movement. Their most systematic and cogent writer, Richard Overton, made plain the sectarians' sense of entitlement to moral autonomy when he described "Every man by nature [as] a King, Priest, and Prophet in his own naturall circuite and compasse, wherein no second may partake, but by deputation, commission, and free consent."<sup>49</sup> To establish the political implications of this view, the Levellers resorted to a natural right of individuals, having discovered that an appeal to Magna Carta and the legal rights of free-born Englishmen could not provide adequate precedents for the rights they now demanded should be enshrined in the law.<sup>50</sup> In order to re-establish the English polity, to reconcile Englishmen one with another, nothing seemed more apposite than an "Agreement of the People" to state the terms on which free-born Englishmen (the transition from being a man to being an Englishman was never explained), equipped with natural rights and, to be sure, natural duties, would consent to be associated in one body politic and to specify the extent and limits of right and trust delegated to their "representative."

THE LEVELLERS, THEN, arrived at a conjuncture of covenant (their term was "agreement") and individual natural right. A parallel, or perhaps derivative, thought is discernible in John Milton. Nothing quite like this conjunction had been asserted before. But, despite the claims of commentators to the contrary, the Levellers did not devise the idea of a state of nature.<sup>51</sup> The genius of Hobbes is seen in this invention of a conceptual device that was (1) coherent with his "compositive method" (the state of nature is a possible generation of civil society), (2) able to mobilize for his case the experience of some eighty years of religiopolitical civil war as well as the humanist *topos* of the post-Golden Age state of man, and (3) capable of serving as a test-bed for assertions of natural right. As Quentin Skinner has shown, Hobbes's theory was widely and quickly recognized as one model of political argument against which the current commonplaces of political thought and attempts to assert contrary positions now had to be measured.<sup>52</sup>

To assume, however, that the Hobbesian pattern of contractual argument swept the board would be erroneous. On the contrary, it is much more accurate to say that henceforth two distinctive contractual languages existed, identifiable by the stable nexi of terms in which each located contract. These languages re-

<sup>49</sup> Robert Overton, *An Arrow against All Tyrants* (1646), Rota Facsimile (Exeter, 1976), 4.

<sup>50</sup> For a description of this process—with something less than profound sensitivity—as "historical mythology to political philosophy," see Christopher Hill, "The Norman Yoke," in *Puritanism and Revolution*, 81. The rest of Hill's account, however, is very serviceable.

<sup>51</sup> Much more to their point was the derivation of the necessity for human law from the fall of Adam; see, for example, Henry Parker, *Observations upon Some of His Majesties Late Answers and Expresses* (2d ed., London, 1642), reprinted in Haller, *Tracts on Liberty in the Puritan Revolution*, 3: 179–80.

<sup>52</sup> Quentin Skinner, "The Context of Hobbes's Theory of Political Obligation," in M. Cranston and R. S. Peters, eds., *Hobbes and Rousseau* (New York, 1972), 109–42, and "Conquest and Consent: Thomas Hobbes and the Engagement Controversy," in G. E. Aylmer, ed., *The Interregnum* (London, 1972), 79–98.

mained distinct even though the bridge afforded by the shared image of a contract as basic to political relationships was often crossed. The history of contractualism subsequent to Hobbes is, indeed, in part a record of the adaptations and modifications of the language Hobbes consolidated. This language spoke of “natural right,” “natural liberty,” “natural equality,” “condition of nature,” “covenant,” and “sovereignty”—best described as the language of philosophical contractarianism, because the theoretical ambitions and the aimed-for generality of thought of those who employed it tended to be greater than that of the alternative language, best described as constitutional contractarianism. This second language set covenant within the terminological context of “fundamental law,” “fundamental rights” or “liberties,” “original contract,” and “ancient” or “fundamental constitution.” In constitutional contractarianism particular positive laws and the institutional inheritance of specific polities were most relevant and important, rather than universal propositions about all men and all polities. This was, in effect, the direct continuation of the kind of thinking found in the works of Hotman, Mornay, Bèze, and Althusius. The contractual language consolidated by Hobbes—the language in which the famous contractualist treatises of Pufendorf, Spinoza, Locke, Thomasius, Christian Wolff, Vattel, Rousseau, and Fichte were written—has received by far the most attention in the secondary literature. Constitutional contractarianism has, by contrast, been almost entirely neglected. But the continuing and quite distinct tradition of constitutional contractualism remained a vital strand in the fabric of the early history of contractarianism.

Locke himself provided some evidence of the continuing vitality of both traditions of contractualist thought when, at the beginning of the eighteenth century, he set out his thoughts on political education.<sup>53</sup> He divided the study of politics into “two parts very different the one from the other.” Locke seems to have had in mind here the age-old distinction between theoretical and practical writing about politics. But the book lists that he recommended to the student of both parts of politics turn out to contain contractualist treatises. For the theoretical part, Locke recommended such literature as Hooker’s *Laws of Ecclesiastical Polity*, Robert Paxton’s *Civil Polity*, his own *Two Treatises of Government*, Sidney’s *Discourses concerning Government*, and Pufendorf’s *De officio hominis et civis* and *De jure naturae et gentium*. For the practical part, he recommended a number of books on English constitutional law and history, all of which either contained explicit arguments from contract or had been interpreted as expressing a kind of constitutional contractarianism in the “controversies between Mr. Petit, Mr. Tyrrel, Mr. Atwood, etc., with Dr. Brady,” to which Locke sent the reader for an appreciation of the works concerned.<sup>54</sup>

<sup>53</sup> J. Axtell, ed., *Locke’s Educational Writings* (Cambridge, 1968), 400–01.

<sup>54</sup> *Ibid.*, 401. For the debates, see J. G. A. Pocock, *The Ancient Constitution and the Feudal Law: English Historical Thought in the Seventeenth Century* (Cambridge, 1957), esp. chap. 8; Carolyn A. Edie, “Succession and Monarchy: The Controversy of 1679–1681,” *AHR*, 70 (1964–65): 350–70; Corrine C. Weston, “Legal Sovereignty in the Brady Controversy,” *Historical Journal*, 15 (1972): 409–31; O. W. Furlley, “The Whig Exclusionists: Pamphlet Literature in the Exclusion Campaign, 1679–81,” *Cambridge Historical Journal*, 13 (1957): 19–36; B. Behrens, “The Whig Theory of the Constitution in the Reign of Charles II,” *Cambridge Historical Journal*, 7 (1941): 42–71;



In these constitutional controversies stretching from the 1670s to the 1690s, clear connections were drawn with contractualist arguments predating the English Civil War. Both Hobbesian and Leveller arguments from natural right and contract were explicitly rejected.<sup>55</sup> Emphasis was placed almost exclusively on justifications in constitutional law for political action and the historic rights of Englishmen. Only Locke, it appears, produced a treatise, intended to contribute to these debates, that relied solely upon notions of natural right, natural law, state of nature, and social contract. Locke's uniqueness in this respect helps account for the initial failure of his *Two Treatises of Government* to achieve the task Locke set himself in publishing: the justification of the Revolution of 1688.<sup>56</sup> The appeal to natural rather than civil rights, to natural rather than fundamental law, to the neglect of any reference to the ancient English constitution were far too radical to the minds of Englishmen concerned to avoid all association with the excesses of the Civil War, king-killing, and the Commonwealth.

The Revolution of 1688 was, of course, justified by the Convention Parliament on the grounds that James II had broken "the original Contract."<sup>57</sup> But this contract was in the language of constitutional contractarianism, not of philosophical contractarianism—a contract that had established the ancient English constitution, that had been renewed in each coronation oath, and that was embodied in English fundamental laws. When confronted by the Commons' resolution that James had broken his contract, the Convention Lords found it quite proper to seek the advice of eminent lawyers. The lawyers pronounced in favor of the Commons' view, several of them rehearsing the arguments to be found in the works of William Petyt,<sup>58</sup> James Tyrrell, William Atwood, and others to whom Locke had referred his readers for a correct understanding of the English constitution.

In these writings, arguments and notions similar to those in the works of Hotman, Bèze, Mornay, and Althusius abound. Indeed, in 1711 Sir Robert Molesworth attempted to base his style of Whiggism on Hotman's authority, and Mornay's *Vindiciae contra tyrannos* was republished in 1689 as part of the case against James II. Opponents of the constitutional contractarians, too, were not slow in pointing out the derivation of their arguments from sixteenth-century Jesuits like Mariana and Parsons as well as other proponents of violence during the French Wars of Religion.<sup>59</sup> There were, of course, differences between these writings of the sixteenth century and those of the late seventeenth century. It

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and M. P. Thompson, "A Note on 'Reason' and 'History' in Late Seventeenth-Century Political Thought," *Political Theory*, 4 (1976): 491–504.

<sup>55</sup> See, for example, R. Ferguson, *A Sober Enquiry into the Nature, Measure, and Principle of Moral Virtue* (London, 1673), 77; and Atwood, *The Fundamental Constitution*, 3.

<sup>56</sup> M. P. Thompson, "The Reception of Locke's *Two Treatises of Government*, 1690–1705," *Political Studies*, 24 (1976): 187–88.

<sup>57</sup> *Journal of the House of Lords* (1688–89), 110.

<sup>58</sup> Petyt, of course, was one of the lawyers called to give advice to the Lords.

<sup>59</sup> Thus, James I declared that "Iesuits are nothing but Puritan papists"; McIlwain, *Political Works of James I*, 126. Also see Peter du Moulin, *A Vindication of the Sincerity of the Protestant Religion in the Point of Obedience to Sovereigns* (London, 1661), chap. 2; Filmer, *Patriarcha*, chap. 1; *Remarks upon the Most Eminent of Our Antimonarchical Authors* (London, 1685); and *The Protestant Jesuite Unmask'd* (London, 1704).

was, for example, of considerable significance that the constitutional contractarians of the late seventeenth century wedded their notions of covenant and compact into a highly articulate theory of fundamental law and the ancient constitution.<sup>60</sup> But their arguments were nonetheless closer to those of the late sixteenth- and early seventeenth-century contractualist writings than to the arguments of Hobbes, Pufendorf, or even Locke. They appealed primarily to “Records, Histories, Law-Books, Charters, and Authentick Manuscripts, from before the fixation of the Monarchy downwards,”<sup>61</sup> rather than to introspection, natural law, or reason.<sup>62</sup> They added the terms “Constitutional Contract” and “Fundamental Contract” to the language of constitutional contractarianism;<sup>63</sup> and, in doing so, they referred back to an earlier language of political practice in which significant inter- and intrapolity disagreements were settled by specific “contracts.”<sup>64</sup>

In the course of their employment throughout the seventeenth century, such references to contract acquired new resonances and associations. It would have seemed nonsense in the mid-sixteenth century to assert that “Fundamental Contract, or Covenant of the Kingdom, or Coronation-Oath,” or simply “the laws made” were “but several Names for the same Thing.”<sup>65</sup> Indeed, it did seem nonsense to both Hobbes and Pufendorf, who explicitly criticized those who “confound *law* and *covenant*.”<sup>66</sup> Yet this proposition was common in late seventeenth-century constitutional contractarianism, where a concern with the intricacies of particular laws and practices and with presenting a “legal” or “historical” case either for or against this or that particular party simply found no room for the niceties of Hobbes’s distinction that “contract is a *promise*, law a *command*. . . . Contracts oblige us; laws tie us fast, being obliged.” Different questions occupied Hobbes and Pufendorf and the constitutional contractarians, and their understandings of contract were accordingly different.

A fuller account of constitutional and philosophical contractarianism, the relationships between them, and their fate in the eighteenth century must await another occasion. We have attempted to do no more than map out the first part of a history that will do more justice to the evidence than previous histories.

<sup>60</sup> See Gough, *Fundamental Law in English Constitutional History*, esp. chap. 10; and Pocock, *The Ancient Constitution and the Feudal Law*, esp. chap. 9.

<sup>61</sup> For example, W. Atwood, *Reflections upon a Treasonable Opinion* (London, 1696), 9.

<sup>62</sup> Whereas constitutional contractarians were frequently prepared to argue their case by any standards, Hobbes, Locke, and Pufendorf were explicit that no amount of historical evidence to the contrary could seriously damage their arguments.

<sup>63</sup> See, for example, R. Ferguson, *Whether the Preserving the Protestant Religion Was the Motive unto, or the End Designed in, the Late Revolution?* in *Somers Tracts*, 3 (London, 1751): 423; and S. Johnson, *Remarks upon Dr. Sherlock’s Book, Intituled the Case of Resistance* (London, 1689), vi. Occasional references to “fundamental contracts,” however, can be found in some of the literature of the Civil War. See, for example, Philip Hunton, *A Treatise of Monarchy* (London, 1643), pt. 1, chap. 2, sec. 4; and R. Baxter, *A Holy Commonwealth* (London, 1649), 436. For note of these, see Gough, *The Social Contract*, 91, 102.

<sup>64</sup> See note 18, above.

<sup>65</sup> See Johnson, *Remarks upon Dr. Sherlock’s Book*; and the Earl of Pembroke’s Speech to the Convention Lords (1689–90), in *A Collection of Parliamentary Debates in England from the Year MDCLXVIII to the Present Time*, 2 (n.p., 1741): 248–49.

<sup>66</sup> Thomas Hobbes, *Philosophical Rudiments Concerning Government and Society*, in W. Molesworth, ed., *The English Works of Thomas Hobbes*, 2 (1839 ed.; reprint ed., Aalen, 1962): 183–85; and S. Pufendorf, *Elementorum juris-prudentiae universalis libri duo* (London, 1660), ed. W. A. Oldfather (London, 1931), bk. 1, def. 13, sec. 2.

Once the presuppositions of Gierkean historiography have been rejected, that evidence begins to yield new and interesting perspectives, some of which we have outlined here. The kind of account we propose, oriented by the concept of the languages of contractualism, suggests a story more complex than the Gierkean and, at the same time, more fecund for future research. Liberated from the constraints of the Gierkean models and sensitive to the intricacies of political languages, the historian of contractarianism in political thought is free to follow the evidence in any of the directions in which it may lead.

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## Superstition to Science: Nature, Fortune, and the Passing of the Medieval Ordeal

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CHARLES M. RADDING

IN THE LATE TWELFTH CENTURY, one of the strongest opponents of judicial ordeals was the Parisian master Peter the Chanter. Part of his attack was theological: God's participation in ordeals could not be relied upon, because, though God could intervene in an ordeal, He could not be compelled to do so by priestly incantation. The Chanter also argued that ordeals violated Biblical prohibitions against tempting God, particularly since many issues decided by ordeals could also be resolved by other, nonmiraculous means. But the greatest part of the Chanter's criticisms were based on experience. He cited cases in which men were known to have been falsely convicted of theft and an instance in which a pilgrim had come home to find that a companion who had returned before him had been subjected to the ordeal of cold water, convicted of the pilgrim's murder, and executed. In addition, Peter pointed out that natural explanations could be found for the outcomes of many ordeals: callused hands were better able to survive tests of hot iron and those who knew how to exhale all of the air from their bodies stood a better chance of victory in the ordeal of cold water.<sup>1</sup>

The Chanter's criticisms were directed against a wide variety of practices that his contemporaries called "judgments of God" (*judicia Dei*). Some of these tests—such as the ordeals by boiling water, immersion, and hot iron—went back to pagan times. Compurgatory oaths, which required the correct repetition of a complicated verbal formula, and judicial duels also apparently dated from traditional Germanic law. Other judgments of God were distinctly Christian. Among these was the Carolingian ordeal of the cross, in which the litigants held their arms out from their shoulders in the shape of the cross; the first to let his or her arms drop lost the case. Diverse as these customs were, all were founded on a

In the preparation of this paper I have benefited from the comments generously offered by many people; Elizabeth A. R. Brown, Robert Lerner, and Clint Goodson in particular have encouraged me through their questions to develop my ideas as fully as possible. An earlier version of this paper was delivered as a talk at Michigan State University.

<sup>1</sup> For a discussion of the Chanter's views, see John W. Baldwin, *Masters, Princes, and Merchants: The Social Views of Peter the Chanter and His Circle* (Princeton, 1970), 323–32, and, more generally, "The Intellectual Preparation for the Canon of 1215 against Ordeals," *Speculum*, 36 (1961): 613–36. For the best general history of ordeals, see Hermann Nottarp, *Götteurteilstudien*, Bamberger Abhandlungen und Forschungen, no. 2 (Munich, 1956); and, for the treatment of ordeals in literature, see R. Howard Block, *Medieval French Literature and Law* (Berkeley and Los Angeles, 1977).

belief that such means revealed a divine judgment on the question at issue. It was this underlying premise, rather than any specific procedure, that the Chanter attacked.

Ordeals had not been without critics before Peter the Chanter. In the ninth century Bishop Agobard of Lyons denounced judicial duels as contrary to the "concordia Christiana." He further argued that it was uncertain whether justice was on the side of the victor or the defeated in any given case; "Christian soldiers," Agobard pointed out, "conquer by dying, not by killing." The problem of inscrutability plagued other kinds of ordeals as well, because a proband could be struck down for sins not related to the case at issue.<sup>2</sup> But these arguments did not question God's participation in ordeals, and they were easily turned upside down: Hincmar of Reims, writing later in the ninth century, cited the inscrutability of divine actions as an argument in favor of trusting ordeals.<sup>3</sup>

Ordeals remained popular into the eleventh century, when leaders of the Gregorian reform movement used them to detect simoniacs and Gregory VII contemplated putting his dispute with Henry IV to the judgment of God.<sup>4</sup> Only in the twelfth century did educated opinion turn decisively against judgments of God. Criticism emerged from several quarters. In England, the legal reforms of Henry II reveal a profound distrust of ordeals. The new procedures to protect property in English law relied exclusively on human testimony, and the measures against those who broke the king's peace punished accused thieves, arsonists, and murderers even when they succeeded at ordeal. Beginning with Ivo of Chartres and Gratian, canonists began to ask whether the ordeal was a licit means of proof; but, except for the Bolognese master Hugucio, who roundly denounced ordeals, most answered cautiously. They usually disallowed those ordeals prohibited by a decree opposing the specific procedure and permitted them in situations where other proofs were not available, and some of the Chanter's own disciples were similarly equivocal. The question was not finally decided until 1215, when the Fourth Lateran Council forbade the participation of priests in ordeals.<sup>5</sup>

IN THE NINETEENTH CENTURY historians generally believed that the history of Western civilization was characterized by the steady progress of enlightenment. Ordeals belonged to the so-called Dark Ages, a period when rational thought was less common than it later became—a period, in fact, with many similarities to primitive societies of the nineteenth century—and, accordingly, there was the

<sup>2</sup> Agobard, *Adversus legem Gundobodi, et impia certamina quae per eam geruntur*, in *Patrologia Latina* [hereafter *PL*], 104: cols. 117–19. Agobard's belief in immanent justice is also shown by his refutation of opinions that certain magicians can cause thunderstorms; only God can make weather, Agobard said, since "apparet . . . hominum Dominum mississe grandinem super eos, quos tali flagello dignos judicaverit." Agobard, *Liber contra insulsam vulgi opinionem de Grandine et Tonitruis*, in *PL*, 104: col. 151.

<sup>3</sup> Hincmar of Reims, *De divortio Lothari*, in *PL*, 125: col. 665, as cited in J. Gaudemet, "Les ordalies au moyen âge: Doctrine, législation, et pratiques canoniques," *Recueils de la Société Jean Bodin*, 17 (1965): *La Preuve*, 2: 99–135.

<sup>4</sup> Colin Morris, "Judicium Dei: The Social and Political Significance of the Ordeal in the Eleventh Century," *Studies in Church History*, 12 (1975): 95–111.

<sup>5</sup> Baldwin, *Masters, Princes, and Merchants*, 323–32.



temptation to place ordeals in the perspective of world history. Henry Charles Lea, among others, pointed out that nearly every society had employed ordeals at some time in its development, just as many societies commonly practiced witchcraft. Lea believed that the rejection of ordeals, as of witchcraft, was an instance of the emancipation of reason "from the cruel and arbitrary domination of superstition and force."<sup>6</sup>

This interpretation leaves much to be desired, particularly the definition of the difference between superstitious and rational thought, but what has provoked the sharpest dissent has been the assurance with which Lea and others categorized some beliefs, and by extension some people, as less rational than others. Lea's interpretation has fallen into disfavor, in part because it resembled arguments in favor of imperialist and racist policies and in part, perhaps, because the wars and inhumanities of the twentieth century have made Westerners less confident that their age is better or more progressive than others. Most scholars today make the assumption that rationality and irrationality are shared rather evenly among all human beings. Rebecca B. Colman, a recent student of ordeals, has explicitly stated this hypothesis, arguing "that men's acts are directed by an untidy mixture of deliberative and emotional processes and of habitual and expedient drives. . . . Reason and unreason, like rational and irrational, are value terms, and much misunderstanding has been generated by their indiscriminate use."<sup>7</sup>

The desire to avoid treating ordeals as superstitions has led many historians to seek alternative explanations. One major effort in this direction has tried to show that ordeals were, in fact, effective instruments of justice. Advocates of this interpretation have argued that fear of perjury compelled some honesty because participants would be reluctant to swear untruthfully. In addition, psychological pressures must have weighed heavily on the guilty, who knew they would face God's infallible judgment; these stresses, it has been suggested, caused many suitors to seek a compromise or to abandon their case before the actual trial. Rebecca Colman has gone so far as to contend that, since most descriptions of ordeals are vague, a community could consciously manipulate the tests according to its presumptions about the truth in the case; larger or smaller stones, for example, could be used in the ordeal of hot water.<sup>8</sup> But surely this point is exaggerated: it is hard to believe that, if such manipulations were taking place on a large scale, suitors would not have complained of unfairness or deviation from custom. Joseph R. Strayer has also pointed out that fear of perjury was not so widespread that clerics did not spend a great deal of time denouncing false swearing.<sup>9</sup> In any event, oaths could be doctored, as was the oath in the twelfth-century poem, *Tristan and Iseult*. Iseult swore that no man had touched her, save the old pilgrim who carried her ashore, when the pilgrim was really Tristan in disguise.

<sup>6</sup> Lea, *Superstition and Force* (Philadelphia, 1892), 590.

<sup>7</sup> Colman, "Reason and Unreason in Early Medieval Law," *Journal of Interdisciplinary History*, 4 (1974): 572.

<sup>8</sup> *Ibid.*, 589–90.

<sup>9</sup> Joseph R. Strayer and Dana C. Munro, *The Middle Ages, 395–1500* (4th ed., New York, 1956), 58.

Peter Brown has recently approached the ordeal from a somewhat different perspective, and his interpretation merits summary as an example of “functionalist” method as well as for its intrinsic interest. Rejecting explanations that treat the ordeal as a feature of “Dark Age” culture that yielded to the progress of rationality (an untenable picture of the ordeal that he has attributed to the twelfth-century critics of the practice, like Peter the Chanter, of whom “modern scholars are still the direct heirs”), Brown has instead asked “what needs led [the people of the Middle Ages] to maintain the ceremony as a satisfactory solution to some difficulties, and then to abandon it in the course of the twelfth century for other means of proof.” He has found that the ordeal appealed to the small groups that made up the society of the early Middle Ages. These groups were extremely susceptible to disruptive conflict—this was the age of the blood-feud—and a government’s ability to confine these conflicts was very limited. In this context, the very length of time consumed by preparations for the ritual was significant, for it gave the parties a chance to resolve their differences. If attempts at mediation failed, the ambiguity of the procedure still allowed the group to determine the outcome: in the trial by hot iron or boiling water, for example, the burned hand was examined after three days, when the normal healing of the tissue was still incomplete and, thus, the determination of the outcome on the basis of physical evidence was ambiguous. Brown has therefore concluded that the ordeal was as “open ended as a Rorschach test” and that the group could project into the event its own unconscious opinion. “What we have found in the ordeal is not a body of men acting on specific beliefs about the supernatural; we have found instead specific beliefs held in such a way as to enable a body of men to act.”<sup>10</sup>

Brown has accounted for the disappearance of ordeals by pointing to two trends that undermined the type of community that had required the ritual. First, “the group itself” changed “consistency.” The reclamation of waste and forest land for cultivation and the relaxation of the need to surround oneself with kin for protection subtly removed the social pressures that demanded conformity and consensus. “Men who needed to care just that little bit less about their neighbors no longer had to go through the more difficult maneuvers of their life in a limelight of supernatural rituals.” The second change involved a “shift from consensus to authority,” in particular the growth of the coercive force of government. Brown has described this development in language appropriate to the conquest of happy villagers by imperialist adventurers: “The ruler was no longer a peacemaker. . . . he was the imposer of law and order. . . . In late twelfth-century England, for instance, law may not have been notably more rational than was the ordeal; but it was trenchant and it was authoritative. The gallows could speak for itself, without mystification.” This shift to a tyrannical authority embraced the exercise of reason by intellectuals—including, one supposes, Peter the Chanter—because their work substituted syllogisms or authoritative texts for the careful and gradual molding of opinion that previously had

<sup>10</sup> Brown, “Society and the Supernatural: A Medieval Change,” *Daedalus*, 104 (1975): 136–40.

been necessary. "In a society where coercive force had come to bear with greater hope of immediate success than in previous centuries, the shrill note of claims for the authority of reason . . . sticks out like a crag in every controversy."<sup>11</sup>

An important aspect of Brown's approach is his feeling that ordeals do not make sense to any impartial observer. This opinion is apparent in Brown's analogy of ordeals to a Rorschach test, which underscores the unconscious origins of the verdicts, and in his treatment of the broadening of communities in the twelfth century, where he has suggested that previous reliance upon supernatural forces has resulted from the pressures for consensus within the earlier, smaller groups. In less claustrophobic circumstances, he seems to have supposed, there would be no reason for men not to see through ordeals. This attitude is typical of functionalist arguments in general: one would not, for example, try to explain acceptance of the law of gravity by the needs of industrial society; to say that it appears to be true and is daily confirmed by experience would be enough. Because belief in ordeals appears so gratuitous, such a denial of everyday knowledge, Brown has tried to account for ordeals solely in terms of their social function. He has therefore had to argue that early medieval groups used the ordeal because they could control it and that the disappearance of the ordeal in the twelfth century corresponded to the break-up of these groups. Neither contention bears close scrutiny.

Brown's argument that the group could control the outcome of the ordeal depends on showing that "an ambiguity lies at the heart of every ceremony." Brown's evidence for this assertion comes not from early medieval sources or from modern medical science but from Peter the Chanter, that twelfth-century propagandist whom Brown has blamed for distorting the ordeal.<sup>12</sup> Brown has simply converted what Peter saw as a fault into a strength, the means by which the group was able to retain control of the proceedings and project its unconscious verdict on supernatural forces. It is, however, at least possible that the judges knew what they were looking for when they interpreted ordeals. Early medieval courts do not seem to have fallen into disagreement when pronouncing the judgment of God, or, if they did, the disputes have not come down to us. Moreover, some of the most popular forms of ordeals—the Carolingian ordeal of the cross, judicial battle, and compurgatory oaths—provided little scope for manipulation by groups.

Brown's description of the shift from consensus to authority is also curiously awry. By Brown's own account, early medieval groups lacked the procedures to reach consensus, and it is hardly certain that litigants would have accepted the authority of group opinion even when it did exist. Ordeals created consensus only through unanimous acceptance of divine judgment—the ultimate in appeals to authority. By contrast, post-Lateran proofs, particularly the jury system, explicitly recognized the value of community opinion, while appeals to reason expressed the belief in any person's ability, when in possession of the facts, to decide for himself without deferring to a superior. Doubtless a ruler of the twelfth or thirteenth century was able to exercise considerable force as long as he re-

<sup>11</sup> *Ibid.*, 142-43.

<sup>12</sup> *Ibid.*, 138, n. 43.

tained the support of his magnates and other subjects, but he was also expected to abide by the laws established by common usage and general consent; the kings of England learned to their dismay that they had to be wary of not meeting the expectations of their subjects. It was, moreover, the intellectuals of the twelfth and thirteenth centuries who firmly placed a ruler in his social context as the head of a community, whereas Carolingian theorists conceived of kingship as arbitrary and of the king as bound by God but not by his subjects.

Although a functionalist interpretation of ordeals might be possible along lines different from Brown's, certain problems appear inescapable. First, functionalist theory depends on supposing that primitive societies can be treated as cohesive social organisms; without this hypothesis, institutions cannot be explained solely as instruments of social cohesion. In the case of the ordeal, however, it is hardly obvious that everybody in society had the same interest. Defeated litigants and their families, at the very least, stood to gain property if not life itself by discrediting the verdicts. We could assume that these individuals were so completely and perfectly socialized that they sacrificed their own survival to preserve community solidarity, but altruistic self-denial scarcely seems a dominant trait in the turbulent communities of the early Middle Ages.

Functionalists must also claim to know better than the people they study what those people mean—a position of great risk for any historian. Participants in ordeals explicitly invited the presence of the supernatural, and they were willing to accept the judgment revealed by the ceremony. Functionalists, in effect, deny that anyone could act on the basis of such preposterous nonsense and assert that people went through the motions in order to impress or reach agreement with their neighbors; instead of a solemn appeal to supernatural judgment, ordeals become a bizarre theatrical performance in which it is difficult to distinguish the actors from the audience. A theory that began as an attempt to acquit poor primitives from accusations of superstition has thus become a claim that no one really believed anything significantly different from what we do today.

The central difficulty of functionalists is not theirs alone. Many historians are too willing to believe that ideas—other people's anyway—are not really important. People act on the basis of their own interests or, alternatively, accept what they are told by their society. Ideas are either rationalizations or instruments of some objective, and intellectuals are the apologists or propagandists of powerful social groups. Not only does this attitude render incomprehensible practices, like the ordeal, that are based on beliefs widely at variance with our own; as philosopher Ernest Gellner has observed, it precludes “making sense of those social changes which arise at least in part from the fact that people sometimes notice the incoherences of doctrines and concepts and proceed to reform the institutions justified by them.”<sup>13</sup> This objection is particularly serious in the case of the

<sup>13</sup> Gellner, “Concepts and Society,” in Bryan Wilson, ed., *Rationality* (New York, 1970), 33. Gellner has further pointed out with regard to anthropologists' functionalist interpretations “that the sympathetic, positive interpretations of indigenous assertions are not the result of a sophisticated appreciation of context, but *the other way around*: that the manner in which the context is invoked, the amount and kind of context, and the

ordeal, for here critics such as Peter the Chanter appear to have played—and certainly thought they were playing—just such a role.

THE GREAT STRENGTH of functionalist interpretations is the illumination of the human interactions that were an important part of the actual performance of ordeals. But, without denying that ordeals had a particular social context, one can try to make sense of the world-view that justified them on its own terms. Paul Rousset has already made an effort in this direction. He observed that belief in ordeal was just one manifestation of medieval acceptance of the idea of immanent justice. Raoul Glaber, for example, had explained a famine as punishment for sin; Herman of Reichenau had described the triumph of the Normans in Italy as a “hidden judgment of God”; and the *Miracles of Saint Benedict* tells how a drought arose out of sin. The ordeal was only a regularization of immanent justice: “The ordeal supposes the miracle, indeed requires it as the only proof capable of opening people’s eyes to the proper judgment. . . . How astonishing is this requirement and acceptance of the miracle in a time when the marvelous was a normal, perhaps a daily occurrence.”<sup>14</sup> Rousset concluded by comparing medieval belief in immanent justice with similar beliefs among children. These speculations have generally been ignored or worse—a recent writer dismissed them as “ill-considered pseudo-anthropological observations”<sup>15</sup>—but they may be founded on accurately noted facts. A colleague of mine who grew up in California remembers a childhood expression, “cheaters never prosper,” that was chanted after a dispute—about, for example, whether a ball was fair or foul—by those winning the replay. A Chicago variant of the saying is “cheater’s proof.”<sup>16</sup> One’s position in an argument is, in other words, vindicated by subsequent events. The replay, in essence, becomes a spontaneous schoolyard ordeal.

These ideas have been studied more systematically by the Swiss cognitive psychologist, Jean Piaget. Piaget found that children often believe that natural events are punishment for moral transgressions. Even in a modern society, in which adults do not endorse these ideas, a lengthy learning process is needed to root them out. One of Piaget’s associates told 167 French children a story about a child who stole apples from an orchard. While running home the child fell into a river when a rotten bridge gave out underfoot. When asked what they thought of the story, most of the youngest children, those under the age of nine,

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way context itself is interpreted depends on prior tacit determination concerning the kind of interpretation one wishes to find. . . . After all, there is nothing in the nature of things or societies to dictate visibly just how much context is relevant to any given utterance, or how that context should be described.” Since it is easy for someone with a functionalist orientation to explain any apparently irrational belief or practice as functional in terms of some context or other, Gellner has concluded that functionalism as an approach is too charitable in the sense that it simply rules out the possibility of finding that beliefs are irrational or unempirical; *ibid.*, 47. For a general critique of functionalist interpretations, see I. C. Jarvie, *Functionalism* (Minneapolis, 1973).

<sup>14</sup> Rousset, “La croyance en la justice immanente à l’époque féodale,” *Le Moyen Âge*, 54 (1948): 241.

<sup>15</sup> Edward Peters, Introduction to Henry Charles Lea, *The Ordeal* [a separately published part of his *Superstition and Force*] (Philadelphia, 1973), xxiv.

<sup>16</sup> I am indebted to Lew Erenberg for calling this to my attention.

said that the child would not have fallen into the water if he had not taken the apples: "It serves him right. He shouldn't have stolen. It serves him right," said one six-year-old. And, when specifically asked if the child who stole the apples would not have fallen into the water had he not stolen, the youngest children said he would not. Children somewhat older tended to feel that he would have fallen in anyway but that he did so because he had stolen; only those near the age of ten were troubled by the contradiction between these two explanations. Finally, at about the age of ten to twelve, Piaget's subjects discarded the idea of immanent justice in favor of mechanical chance: the thief fell because the bridge was rotten and would have fallen in even if he had not taken the apples.<sup>17</sup>

If ideas of immanent justice arise without teaching by adults or support from social institutions, resort to a functionalist explanation is not necessary to account for their appearance in the early Middle Ages; it is enough to say that social usage and common opinion encouraged the retention of these ideas into adulthood. A better question to ask, therefore, is how Europeans learned to distrust ordeals in the twelfth century. In searching for an answer, the parallel between the intellectual development of children in modern society and the criticism of ordeals in the twelfth century becomes significant. Colin Morris has commented that believers in ordeals had no very clear idea of how they worked: there is "almost complete silence" on the subject in the sources.<sup>18</sup> But by the twelfth century Peter the Chanter was acutely interested in the means by which the ordeal was supposed to work—in his concern for the way God's participation was invoked and in his treatment of frauds. The Chanter also commented ironically on a kind of dual causality assumed by participants in trial by battle by asking why litigants invariably chose their champions according to their skill in arms. "Why did they not choose decrepit or old men so that God could clearly show the miracle?"<sup>19</sup> Piaget observed that similar concern with the causal mechanism of immanent justice only appears among "those who no longer believe the physical universe functions like a policeman"; younger children, like those who reacted to the story of the bridge, are quite willing to propose two mutually independent explanations of a single event.<sup>20</sup> Since both in children and in the men of the Middle Ages the breakdown of beliefs in immanent justice is closely tied to a changing conception of the physical universe, these beliefs need to be set in the context of other attitudes toward the world.

Piaget placed belief in immanent justice in a world-view that he called egocentric, using this term to convey both the individual's sense of being at the center of things and his inability to imagine points of view different from his own. A simple example is a child's belief that clouds or the sun follow him when he walks; only when he realizes that clouds seem to follow everyone at once does he accept the true explanation that the clouds are very high up. Appearances,

<sup>17</sup> Piaget, *The Moral Judgment of the Child*, trans. M. Gabain (1932; New York, 1948), 251–62.

<sup>18</sup> Morris, "Judicium Dei," 101.

<sup>19</sup> *Verbum Abbreviatum*, in *PL*, 205: 548B.

<sup>20</sup> Piaget, *The Moral Judgment of the Child*, 257–58.



therefore, are taken as reality.<sup>21</sup> Mountains are assumed to look the same on the other side as they do on the side in view. When water from one of two identical beakers is poured into a container of a different shape, the child will say that the quantities are no longer equal because they now look different.<sup>22</sup> Names are treated as real properties of objects: when she was three, my daughter Sarah distinguished carefully between a shoe and a sneaker and did not permit me to confuse them. Children also treat qualities as belonging to objects instead of to categories applied by humans to objects: when she was four, Sarah knew she was larger than babies and smaller than adults, but she did not think of herself as belonging to a series of heights; she was “medium-sized.”<sup>23</sup>

It might seem that such complete reliance on appearances would result in a conception of the world as impossibly complex and disordered, but in practice the reverse occurs. To the egocentric individual, the world appears perfectly ordered, and every feature or occurrence is treated as meaningful. But egocentric order does not result from the uniform operation of natural physical laws, because physical laws, like moral laws, have many exceptions.

When children between 5 and 8 are asked whether the sun could go away if it wanted to, they always answer that it could: if it does not go away it is because it “has to shine a little longer,” or because “it has to lighten us during the day.” Clouds cannot go because they show us the way, etc. In short, if there are natural laws at work, it is not because the bodies in question are physically determined; they could perfectly well evade the law if they wished to. It is simply that they are obedient.<sup>24</sup>

One consequence of this world-view is that miracles do not have the character of violations of physical laws; rather, they are unforeseeable but not otherwise surprising events. Piaget concluded that only for a “rational mind” does the question of “miracle” arise, “because it is contrary both to natural regularities and to fortuitous fluctuations. For the ancients, on the contrary, miracle (in the etymological meaning of ‘marvel’ or ‘wonder’) was a natural thing, and for the primitive everything is, in fact, a miracle.”<sup>25</sup>

Because egocentric individuals are incapable of rationally analyzing appearances to distinguish between natural regularities and fortuitous coincidences, they frequently believe that they can influence the physical world through correctly performed rituals. The belief that, by walking, a person can make clouds move is a simple instance of this attitude. Other examples are the beliefs that, by making a shadow, a child can cause night to come or that, by choosing stones of the right color and shape for the bottom of a flower pot, an individual can make a plant grow. Piaget has pointed out that to say that these rituals are

<sup>21</sup> Piaget, *The Child's Conception of Physical Causality*, trans. M. Gabain (London, 1930), 246–47.

<sup>22</sup> Piaget and Bärbel Inhelder, *The Psychology of the Child*, trans. H. Weaver (1966; New York, 1969), 97–99.

<sup>23</sup> For Piaget on names, see his *The Child's Conception of the World*, trans. J. and A. Tomlinson (London, 1929), 61–87; and, on seriation, see Piaget and Inhelder, *The Psychology of the Child*, 101–02. This brief summary of Piaget's theory also draws on Jean Piaget and Bärbel Inhelder, *The Origin of the Idea of Chance in Children*, trans. Lowell Leake, Jr., Paul Burrell, and Harold D. Fishbein (1951; New York, 1975). For a good introduction by Piaget and Inhelder, see their *The Psychology of the Child*. And, for a helpful guide to Piaget's own studies, see J. H. Flavell, *The Developmental Psychology of Jean Piaget* (Princeton, 1963).

<sup>24</sup> Piaget, *The Child's Conception of Physical Causality*, 274.

<sup>25</sup> Piaget and Inhelder, *The Origin of the Idea of Chance*, 221.

projections of wishes is not sufficient, because such an interpretation “seems to claim that, if a desire has an exceptional value, belief in its necessary realization must follow”; moreover, wishes are able to find their expression in magic because egocentric cognitive structures cannot differentiate the objective and the subjective. Gestures or names are real and powerful. Thus, although “magical actions are, to the observer, symbols . . . , to the subject they are effective, precisely because they are not yet symbolic and because they participate in things.”<sup>26</sup>

Egocentricity is not merely a matter of inexperience in the world, nor is increased experience alone enough to account for the lessening of egocentricity as the child grows up. Piaget found that children could not easily be dissuaded of their ideas by counterexamples. If a child believes that going to sleep makes it dark, for example, he will continue to hold that belief even when someone objects that sleeping in the afternoon does not bring night.<sup>27</sup> Instead of experience, the development of what Piaget called “operations,” the ability to manipulate phenomena mentally, is crucial. An early achievement of the operational child is the recognition that water does not change volume by being poured into a container of a different shape. Other operational abilities include the seriation of large numbers of objects (young children have difficulties with more than three), the addition of quantities, and the establishment of stable groupings. These logical operations, which permit classifying and analyzing sensory data, have the effect of decentering the conception of the world. Chance occurrences become admissible, if only as events that are not predictable, and finalist explanations gradually disappear.<sup>28</sup>

In modern Western children the operational stage is usually reached at about the age of six or seven, when the conservation of volume of liquids, despite changes of shape, is mastered. Formal operations, the ability to think critically about thought itself and to consider more than one variable at a time, develops in Western children at about the age of ten to twelve. In some other societies, however, operational logic develops only much later, if at all. In some African societies, children rarely master conservation of volume, and even some adults remain unable to recognize constant volume.<sup>29</sup>

In the absence of well-developed operational skills, many egocentric notions about the world can persist into adulthood. An instance of pre-operational

<sup>26</sup> Piaget, *The Child's Conception of the World*, 152, 162.

<sup>27</sup> *Ibid.*, 292.

<sup>28</sup> Piaget and Inhelder, *The Psychology of the Child*, 93–103.

<sup>29</sup> For a recent survey of cross-cultural research, see P. R. Dasen, “Cross-Cultural Piagetian Research: A Summary,” in J. W. Berry and P. R. Dasen, eds., *Culture and Cognition: Readings in Cross-Cultural Psychology* (London, 1974), 409–23. Dasen has tentatively concluded that, “in all cultures studied so far, some or all individuals reach the stage of concrete operations, although usually at a later age than middle-class Europeans. The fact, however, that some individuals, even of adult age, continue to show a pre-operational type of reasoning, and that some qualitative differences are being reported, indicates that environmental factors may be more important than Piaget seemed to hypothesize in his earlier writings.” *Ibid.*, 421. For a somewhat differently balanced survey, see Michael Cole and Sylvia Scribner, *Culture and Thought: A Psychological Introduction* (New York, 1974), esp. 146–56. Many psychologists, of course, reject Piaget's theories in whole or in part, but objections of one sort or another have been offered to nearly every major psychological theory, Freud's included. Piaget's theories do offer a plausible way of understanding historical phenomena that have proved intractable to other approaches.

thought of particular interest here is the Melanesian practice of cargo-cults, which have been analyzed in terms of cultural egocentricity.<sup>30</sup> The cults began early in this century when the natives—or indigenes—of the area converted to Christianity in the hope that, by adopting European religious ritual, they would obtain the manufactured goods, or cargo, that the Europeans had. The indigenes initially attended church and gave up polygamy and cannibalism, but the cargo failed to arrive. Following the advice of visionaries, the natives then destroyed their traditional ritual objects, attended church more regularly, and abandoned sorcery, still to no effect. Finally, convinced that the missionaries had withheld from them the truly efficacious rituals, the indigenes tried to guess the secret. One group put bottles of flowers on the table; another added bathing in hot water to their traditional fertility rites; others listened to flagpoles as though they were radio aerials, dug up geodesic markers under which the cargo was thought to be hidden, and tried to elect Lyndon Johnson as assembly representative from New Hanover. Diverse as these efforts were, they shared a belief common among small children of every society that the physical world can be manipulated by correctly performed ritual: “The belief that the ritual must be carried out precisely right in order to achieve results is . . . a cardinal belief of the Indigenes’ techniques for controlling nature. . . . In order to have magical power, the symbolic act must be carried out precisely as it had been when effective previously.”<sup>31</sup>

Many early medieval religious ideas betray egocentric notions of the world similar to those of the cargo-cults. Just as the cultists assimilated Christian beliefs and Western behavior to their own pre-operational logical schemes, medieval ordeals retained a formidable array of ritual even after they had been adapted to Catholic doctrine. The tools of the ordeal were themselves magical and had to be preserved with great care, if the magic were not to be lost. The abbot of Saint Wandrille of Rouen, for example, had to ask the archbishop of Rouen to consecrate a new ordeal iron because one of the monks, “out of ignorance and a certain simplicity,” had employed the old one for mundane purposes. At Canterbury Cathedral the pool of holy water used for baptism was also used for ordeals by immersion. And the proband of the ordeal himself was often cut off from everyday activities by three days of fasting during which he was shaved and stripped of his usual clothes and amulets.<sup>32</sup>

The ordeal itself, of course, had to follow a carefully worked out ceremony. A tenth-century version of the ordeal of cold water, for example, instructed the priest to receive the probands, lead them into the church, and offer them mass with the words, “May this body and blood of our Lord Jesus Christ be with you today at the trial.” When the mass was complete, the priest was to consecrate water and make it holy, take it with him to where the probands were to tried, and offer it there for each of them to drink. Then he was to conjure the water,

<sup>30</sup> R. W. Lidz, T. Lidz, and B. G. Burton-Bradley, “Culture, Personality, and Social Structure: Cargo-Cultism—A Psychosocial Study of Melanesian Millenarianism,” *Journal of Nervous and Mental Diseases*, 157 (1973): 370–88. I am indebted to David Freedman for this reference.

<sup>31</sup> Lidz, Lidz, and Burton-Bradley, “Culture, Personality, and Social Structure,” 383.

<sup>32</sup> Morris, “Judicium Dei,” 100; and R. W. Southern, *St. Anselm and His Biographer* (Cambridge, 1963), 265.

strip the probands of their clothes, have each of them kiss the gospels and the cross, sprinkle each with the holy water, and throw them one by one into the water.<sup>33</sup>

Some scholars have argued that the social utility of the ordeal depended upon correctly performing each stage of the ritual, because the lengthy process allowed ample time to settle a dispute by compromise, but no one has sufficiently stressed that this social utility was grafted onto a psychological reality. The elaborate precautions were necessary to preclude the interference of malign spirits, who might otherwise skew the results. In some rituals priests offered prayers to set a barrier against Satan's interference; in the ordeal by water just described the proband was cautioned not to continue if he knew his cause was unjust "lest some *maleficium* prevail against him." Confronted with such dangers, one would no more have thought of conducting an ordeal without the proper ritual than someone today would take an automobile trip without checking the fuel and the spare tire: the precautions may not be necessary, but there is no other way to be sure.

Although the ordeal is the most obvious of the rituals that were used in the early Middle Ages to control the physical world, it was not the only instance of such efforts. Monastic maledictions, which Lester K. Little has recently discussed, shared many of the characteristics of the ordeal. The purpose of the maledictions was to call down a variety of misfortunes upon those who offended the monks. The liturgy enumerated with care and thoroughness what ills the monks intended should befall their enemies: disease, starvation, and death in various ways. To assure the effectiveness of their curses, the monks employed verbal formulas that they received—or thought they received—via popes and saints. The monks of Saint Martial of Limoges considered the precise wording so important that they copied the text of their malediction into a Bible along with other precious documents. And they invoked these curses in ceremonies reminiscent of sympathetic magic; candles evidently were snuffed out by the monks as the curse concluded, "And thus may all memory of them be extinguished forever and ever." Although these maledictions can be seen as mere prayers for the anathematization of sinners, Little has suggested that a "simpler reading would see the curses as real actions."<sup>34</sup>

Miracle stories are also full of simple but efficacious rituals. Gregory of Tours reported that, when a fly tried to land in the cup of a priest of Poitou, the priest changed the cup to his left hand and made a cross with his right. Then the priest held the cup up high and poured its contents on the ground, "for it was plain that it was a device of the enemy." The potency attributed to the sign of the cross is also revealed by a story of Gregory the Great about a priest of Ferentino who could make snakes explode by making the sign of the cross over them.<sup>35</sup> The relics of saints were seen as another source of magical power. Greg-

<sup>33</sup> Petrus Browe, *De Ordaliis*, 2 (Rome, 1933): no. 1.

<sup>34</sup> Little, "The Use of Monastic Maledictions," *Annales: Économies, sociétés, civilisations* (forthcoming).

<sup>35</sup> Gregory of Tours, *Book in Honor of the Martyrs*, chap. 103; and Gregory the Great, *Dialogues*, trans. O. J. Zimmerman (New York, 1959), bk. 3, chap. 35 (p. 176).

ory of Tours credited an amulet containing the relics of an unknown saint with sparing him from being rained on, and he claimed that he could cure headaches and other disorders by touching the affected part of the body to the tomb of Saint Martin. In the absence of physical relics, it was often enough simply to invoke the saint by name. Guibert of Nogent related how a sorcerer and his pupil, from whom Guibert claimed to have gotten the story, took a roast cock out to a fishpond as an offering to the devil. As the two stood by the pond, the sorcerer called on the devil, who appeared in the form of a whirlwind and seized the cock, but the pupil was so frightened that he called upon Saint Mary. "When the Devil heard the name of that powerful Lady, he fled with the cock, but he was unable to carry it off, and the next day it was found by some fishermen on an island in the fishpond."<sup>36</sup>

The interest of miracle stories does not, however, reside merely in the potency that these medieval authors attributed to ritual; the very ordinariness of the events that they accepted as miracles needs to be explained. Almost anything, it seems, could be seen as a miracle, a judgment, or the action of a demoniacal spirit—a fly tries to land in a cup; clouds pass by without rain; a cooked fowl is blown out of a man's hands and ends up on a nearby island. Each of these incidents could have been explained as unexceptional or chance occurrences had the chroniclers been inclined to be skeptical. Such skepticism as they showed, however, is not the sort to inspire confidence in their critical judgment. Gregory the Great never saw the Ferentine priest Amantius make a snake burst; his personal experience was that Amantius had calmed down a mental patient in one of Gregory's hospitals so that the man did not disturb the rest of the patients for the night. By the standards of modern faith healers, the success was rather slight, but it was enough for Gregory. "From this one miracle we learned to put our faith in everything we heard about Amantius."<sup>37</sup>

A PSYCHOLOGICAL NEED TO FEEL PROTECTED against a hostile environment does not account for early medieval beliefs in the supernatural. The mental structures that permitted the easy projection of wishes vivified guilts and fears with equal facility; if God and His saints were close at hand, the world was also populated with demons and malign spirits. Ordinary misfortunes—drought, accident, disease, the rapacity of one's neighbors—took on the character of divine judgments of a person's moral worth. Thus, a trivial event could call into question the state of one's soul. Whether early medieval beliefs in the supernatural were on balance more productive of comfort or anxiety is hard to say.

<sup>36</sup> Gregory of Tours, *Book in Honor of the Martyrs*, chap. 83, and *Four Books on the Miracles of St. Martin*, bk. 3, preface; and Guibert of Nogent, *Self and Society in Medieval France: The Memoirs of Abbot Guibert of Nogent*, ed. and trans. John Benton (New York, 1970), 116–17 (bk. 1, chap. 26).

<sup>37</sup> Gregory the Great, *Dialogues*, 176. F. H. Dudden, who has discussed this incident, concluded that "Gregory had no capacity either of weighing or testing evidence brought forward by others, or for drawing correct inferences from what fell within his personal observation. Further, since Gregory was certainly the most intelligent Roman of his age, it is safe to assume a similar or even greater incapacity to the original authorities he cites." Dudden, *Gregory the Great*, 1 (London, 1905): 343.



If we refuse to dismiss miracles and ordeals as simply expressions of unconscious wishes, two features then stand out as typical of the reasoning that persuaded people to believe in the supernatural. First, there is the egocentric assumption that everything that occurs must be meaningful. Countless miracle stories can best be understood as efforts to explain common but unpredictable events in terms of ultimate causes, and a little ingenuity can discover edifying morals in the most unlikely circumstances. When the Church of Saint Martin of Tours burned early in the tenth century, many were disappointed that Martin had performed no miracle to save the building. But Odo of Cluny, then a canon at Tours, argued that the fire itself was the miracle: it gave those associated with the church a reminder to repent and provided them with an opportunity to win credit in heaven by participating in the rebuilding of the church.<sup>38</sup>

Second, the ready perception of supernatural forces in everyday occurrences suggests that the miraculous and ordinary were on the same level of experience, distinguished only by the rarity of the miracle. Isidore of Seville tried to convey this idea in one of the few early medieval definitions of miracles. They are not, he wrote, "contrary to nature, because they are created by the divine will, and the will of the Creator is the nature of each created thing. . . . A miracle, therefore, does not happen contrary to nature, but contrary to nature as known."<sup>39</sup> Just as Piaget found that children could not grasp the idea of physical necessity, Isidore seemed to say that physical law does not exist apart from God's will; in a sense, all events are equally miraculous because, whether common or uncommon, they express God's will. Because God already is everywhere, his intervention in conflicts with demons can be counted on and his participation in ordeals is nothing out of the ordinary.

These ways of understanding experience are apparent as early as Saint Augustine. According to his *Confessions*, for example, Augustine regularly assumed that his conversion was guided by God's regular intervention in his life. His abandonment of astrology, his disillusionment with the Manichees, and his departure for Rome were among the incidents Augustine interpreted as God's actions. Consistent with this approach was Augustine's steady refusal to make any distinction between miracles and natural events; indeed, one of his favorite arguments against skeptics pointed out ordinary phenomena that were no harder to understand than the miracles of the saints. Isidore's definition of miracles paraphrased such a discussion from Augustine's *City of God*, and Augustine made the same point in some of his less formal works.<sup>40</sup> In one of his letters Augustine attacked those who doubted Christ's divinity because the incarnation of God, he said, was no more inexplicable or impossible than many things in daily experience. "As for instance the force of seeds, which has what numbers, such vital,

<sup>38</sup> Odo of Cluny, *Sermo* 4, in *PL*, 133; cols. 729-49.

<sup>39</sup> Isidore of Seville, *Etymologiae sive originum*, ed. W. M. Lindsay (Oxford, 1911), bk. 11, 3, 1-2: "Portenta esse Varro ait quae contra naturam nata videntur: sed non sunt contra naturam, quia divina voluntate fiunt, cum coluntur Creatoris cuiusque conditae rei natura sit. Unde et ipsi gentiles Deum modo Naturam, modo Deum appellant. Portentum ergo fit non contra naturam, sed contra quam est nota natura." For the translation, see E. Brehaut, *An Encyclopedist of the Dark Ages, Isidore of Seville* (New York, 1912), 69.

<sup>40</sup> Augustine, *Confessions*, bk. 4, chap. 3, bk. 5, chaps. 5, 7, and *City of God*, bk. 21, chap. 8.



such efficient, such hidden powers, such building in their smallness of something great; who can understand or describe it? God therefore made himself man without seed who in the nature of things makes seeds without seeds.”<sup>41</sup> Augustine’s equation of the miraculous and the ordinary leaves no room for uninspired events. “Even in the nature of things known to all are occurrences equally marvelous, and they would be a great wonder to all who consider them if men marvelled at any marvels except the uncommon.”<sup>42</sup>

M.-D. Chenu has brilliantly shown how in the twelfth century these attitudes were supplanted by a new sense of the physical world. The spreading use of the word *universitas* (universe) is one indicator of this new awareness. “The word came spontaneously into use whenever men were thinking of the homogeneity of natural phenomena.”<sup>43</sup> Twelfth-century authors used a remarkable variety of images to convey their sense of the wholeness of the universe—a zither, a machine, the world-soul, and particularly Lady Nature—but in each case the metaphor pointed toward the operation of natural forces that did not require the regular intervention of God.

These images were not merely fashionable ways to express old ideas; they reflect new premises used in analyzing events. In the commentary on the *Hexaemeron* that Abelard wrote at Heloise’s request, he thought that, although natural forces were the will of God, they had been relatively fixed since the creation. “For we are accustomed to consider the force of nature . . . as the preparation or constitution sufficient to making certain things without miracles; thus, those things which are made through miracles we acknowledge to be done either against or above nature, since they never occur . . . unless God conferred a certain new force to things themselves. . . . Nature, therefore, we call the power things possess, conferred upon them in that first preparation, sufficient for them to give birth to something, that is to effect it.” Abelard not only limited the role of the miraculous in daily life; he tried to explain the process of creation itself as much as possible in natural terms. Heaven and earth divided, he wrote, because one was formed of lighter elements (air and fire) and the other of heavier. He compared the forming of animals from the four elements of the world to the growth of chicks from the same elements contained in an egg, and he was so greatly perplexed by the second day’s reference to water over the heavens—a lighter element over a heavier—that he offered a

<sup>41</sup> Augustine, Letter 137, 10, in *Corpus Scriptorum Ecclesiastorum Latinorum* [hereafter CSEL], vol. 44: *Epistolae*, ed. Al. Goldbacher, pt. 3, 109: “Quid autem non mirum deus facit in omnibus creaturae, nisi consuetudine cotidiana viliissent? . . . Sicut ipsa vis seminum quos numeros habet, quam vivaces, quam efficaces, quam latenter potentes, quam in parvo magna molientes, quis adeat animo, quis promat eloquio? Ille igitur sibi sine semine operatus est hominem, qui in rerum natura sine seminibus operantur et semina.” Augustine also considered a wide variety of other phenomena to be miracles, including the grafting of olive branches and the operation of magnets, and he cited such phenomena as an argument against those who doubted the miracles of the saints. See J. Sumption, *Pilgrimages: An Image of Mediaeval Religion* (London, 1975), 65–66; and Peter Brown, *Augustine of Hippo: A Biography* (London, 1967), 415–18. It would be interesting to explore the extent to which the Greek concept of natural order had been lost by late antiquity, but such an investigation would take this discussion far afield.

<sup>42</sup> Augustine, *City of God*, bk. 21, chap. 8.

<sup>43</sup> Chenu, *Nature, Man, and Society in the Twelfth Century*, trans. and ed. J. Taylor and L. K. Little (Chicago, 1968), 6 (originally published as *La théologie au douzième siècle* [Paris, 1957]).

number of suggestions as to how this could have occurred.<sup>44</sup> Whereas Augustine assimilated the natural to the miraculous, Abelard tried to reduce the miraculous—including the days of creation—to everyday causes.

Abelard's approach is the more striking because he began from theological principles virtually identical to those of Augustine. He assumed that the will of God (*voluntas Dei*) guides the events of the world and considered nature an instrument of God's revelation. Abelard's similarity to Augustine on these points is so great that Tullio Gregory has recently argued that Abelard was essentially traditional, that "he did not go beyond the Augustinian conception of nature."<sup>45</sup> Gregory's analysis, however, overlooks the important differences in logical method or structure between Abelard and Augustine. To Augustine, all explanations went back to the will of God, and any regularity of nature derived from its conformity to rules enforced by God; Abelard, however, conceived of causation in terms of physical necessity and, where possible, generally avoided introducing any other causes. In effect, Abelard employed Augustine's terminology to build a philosophical structure at variance with that of Augustine.

Andrew of Saint Victor, among other twelfth-century thinkers, resembled Abelard in his reluctance to resort to a miraculous interpretation of the Bible. "In expounding scripture," he wrote, "when the event described admits of no natural explanation, then and only then should we have recourse to miracles."<sup>46</sup> Andrew's method was different from Abelard's because Andrew primarily tried to recapture the meaning intended by the author of the text and was not greatly interested in causality. Even here, however, there was an important shift away from supernatural explanations, for Andrew assumed the words of the texts were chosen by a human author and not, as Gregory the Great had thought, dictated by the Holy Spirit. Before long, skepticism about the supernatural turned to contemporary phenomena. In 1162 Bishop Manasses of Orleans expressed doubts about the relics of Saint Genevieve, and John of Salisbury later had to answer those who discounted Thomas à Becket's miracles as "men's fantasies"; Abelard himself had to leave Saint Denis after questioning the authenticity of relics preserved at the abbey. By the end of the twelfth century, philosophers were feeling obliged to answer "certain impious men" who argued that "things contrary to nature cannot occur," that God himself could not modify the laws of nature. And that position had to be condemned again in 1277.<sup>47</sup>

Increasing skepticism toward miracles is also apparent in the development of procedures to regulate the canonization of saints. Before 1100 virtually no effort

<sup>44</sup> Abelard, *Expositio in Hexameron*, in *PL*, 178: 746: "Deinceps vim naturae pensare solemus, tunc videlicet rebus ipsis jam ita praeparatis, ut ad quaelibet sine miraculis facienda illa eorum constitution vel praeparatio sufficeret. Unde illa quae per miracula fiunt magis contra vel supra naturam quam secundum naturam fieri fatemur, . . . nisi quandam vim novam rebus ipsis Deum conferret. . . . Naturam itaque dicimus vim rerum ex illa prima praeparatione illis collatam ad aliquid inde nascendum, hoc est efficiendum sufficientem." My thanks to Donald Sutherland for suggestions about the translation.

<sup>45</sup> Gregory, "Considérations sur ratio et natura chez Abélard," in *Pierre Abelard/Pierre le Venerable: Les courants philosophiques, littéraires, et artistiques en occident au milieu du XII<sup>e</sup> siècle* (Paris, 1975), 575–76. Also see M. Gandillac's comments in the subsequent discussion, in *ibid.*, 582–83.

<sup>46</sup> Andrew of Saint Victor, Commentary on Ezechiel, as quoted in Beryl Smalley, *The Study of the Bible in the Middle Ages* (2d ed., Oxford, 1952), 144.

<sup>47</sup> Chenu, *Nature, Man, and Society in the Twelfth Century*, 14–15.

was made to control the proliferation of cults; bishops nominally had the authority to forbid or approve the establishment of shrines, but they rarely employed their power. Dissatisfaction with this situation, together with the heightened prestige of the papacy in the eleventh and twelfth centuries, led to the centralization of the power to canonize saints. The classic statement of papal prerogatives of canonization, later incorporated into Gregory IX's collection of decretals, was a bull of Alexander III forbidding the veneration of a Swedish saint who reputedly was killed while intoxicated. Celestine III explicitly justified papal monopoly of canonization by referring to the powers of the Roman church to correct errors and resolve ambiguities.<sup>48</sup>

The popes themselves showed growing concern in the course of the twelfth century about validating the miracles of proposed saints. Urban II had been content in 1099 to ask for a written account of a proposed saint's life and miracles, but by the 1170s the miracles of even celebrated holy men were treated with great care. The canonization of Saint Bernard was delayed ten years by Alexander III after the Council of Tours first sought it in 1163, though the postponement may have been due to the pope's desire to avoid offending those who had presented other candidates for canonization at the same council. A clearer instance of papal caution is the case of Thomas à Becket, who was also canonized in 1173. Alexander, not satisfied with a book about Becket's life and miracles, instructed his legates personally to examine witnesses to the alleged miracles. Innocent III completed the formalization of church procedure in such matters by requiring his representatives in the case of Gilbert of Sempringham not only to amass evidence of miracles but to take down the testimony of trustworthy witnesses to their authenticity.<sup>49</sup> The standards were intended to be rigorous. Cardinals examining the miracles of Saint Edmund of Abington in 1247 remarked that few Church Fathers would have been canonized had this procedure been followed for them.<sup>50</sup>

Thirteenth-century evidence suggests that even popular readers demanded more from their miracles than had the comparatively well-educated Gregory of Tours or Guibert of Nogent. The *Stella Maris* of John of Garland illustrates this tendency in its successive versions of a miracle. A Jew of Constantinople defiled an image of the Virgin; after a Christian cleaned the image and placed it in a church, oil began to flow from it. The earliest version, that of Adamnan (ca. 623–704), said simply, "What the Jew did afterward, how he lived, or how he ended his life, is unknown." A twelfth-century collection was more definite: "It is believed that, because of the crime which he committed against Christ and his mother, evil spirits removed him from the sight of man." A later version, spuriously citing Saint Jerome as a source, converted this hypothesis to a certainty: "And when he had done this, in the sight of everyone, devils came and carried him off, soul and body, to hell." F. E. Wilson, who has traced this evolution, has

<sup>48</sup> E. W. Kemp, *Canonization and Authority in the Western Church* (Oxford, 1948), 96–99.

<sup>49</sup> *Ibid.*, 67–69, 83, 86–89, 104–05; and Raymonde Foreville, ed., *Un procès de canonisation à l'aube du XIII<sup>e</sup> siècle (1201–1202): Le livre de Saint Guibert de Sempringham* (Paris, 1943).

<sup>50</sup> E. Martène and V. Durant, *Thesaurus novus anecdotorum*, 3 (Paris, 1717): col. 1851, as cited in Sumption, *Pilgrimages: An Image of Mediaeval Religion*, 149.

commented that “the development of the devil’s role in this legend is instructive in relation to the whole history of the medieval miracle.”<sup>51</sup> For our purposes it is enough to note that from the twelfth century authors felt the need both to provide greater authentication of the events and to support the claim of a miracle with more strikingly unnatural events.

One effect of the increasingly rigorous scrutiny given to miracles was to increase greatly the number of supernatural cures in proportion to other kinds of miracles. Of 123 miracles recorded in the early eleventh-century collection of Sainte Foi, only 43 percent were cures; of 89 miracles in the late eleventh-century collection of Saint Vulfran, 75 percent were cures. The procedures instituted by Innocent III had the result of almost entirely discarding prodigies, revelations, and divine punishments. Of the 30 miracles of the official collection of Saint Gilbert of Sempringham, only one—concerning winds favorable for sailing to Normandy—was not a cure; of 26 miracles in a supplementary collection, all except one—a vision—were cures.<sup>52</sup> And the continued belief in supernatural cures itself reflects, as R. C. Finucane observed, “the fact that practically nothing was known about health and sickness in the ‘scientific’ sense.”<sup>53</sup>

The decline of belief in immanent justice and divine punishments is also apparent in the twelfth-century revival of classical Fortuna and her wheel. Fortuna was known to the early Middle Ages through Boethius’ *Consolation of Philosophy*, but the image had not caught on: Pierre Courcelle found only one pictorial and three literary representations of Fortuna from before 1100.<sup>54</sup> The unpopularity of Fortuna probably derived from patristic times. Lactantius, for example, denounced those who believed “in fortune as if a certain goddess sports with human affairs by varied accidents, since they do not know whence good and evil came to them.” Augustine similarly disliked the idea of fortune, because it seemed to him the opposite of divine justice. “It pains me . . . to hear fortune named when I see men have the bad habit of saying ‘Fortune wished this’ when they ought to say ‘God wished this.’”<sup>55</sup> Isidore of Seville, following Lactantius, also portrayed fortune as the opposite of justice: “They say Fortune has its name from fortuitous events, as if a certain goddess sports with human affairs by varied accidents and fortuitous events; from which they call it blind,

<sup>51</sup> Wilson, *The Stella Maris of John of Garland* (Cambridge, Mass., 1946), 173.

<sup>52</sup> P. A. Sigal, “Maladie, pèlerinage, et guérison au XII<sup>e</sup> siècle: Les miracles de Saint Gibrien à Reims,” *Annales: Économies, sociétés, civilisations*, 24 (1969): 1527 n. 1; and Foreville, ed., *Un procès de canonisation à l’aube du XIII<sup>e</sup> siècle*, xli–xliii.

<sup>53</sup> Finucane, “The Use and Abuse of Medieval Miracles,” *History*, 60 (1974): 6.

<sup>54</sup> Courcelle, *La “Consolation de Philosophie” dans la tradition littéraire* (Paris, 1967), 135, 141–43.

<sup>55</sup> Lactantius, *Institutes*, bk. 3, 28, 6, in *CSEL*, 19: 264, 24: “Credunt esse fortunam quasi deam quandam res humanas variis casibus illudentem, quia nesciunt unde sibi bona et mala eveniant.” Augustine, *Contra Academicos*, bk. 1, pt. 1, 1, in *CSEL*, 63: 3, 17: “Etenim fortasse quae vulgo ‘fortuna’ numonatur, occulto quodam ordine regitur nihilque aliud in rebus ‘casum’ vocamus, nisi cuius ratio et causa secreta est.” And Augustine, *Retract.*, ed. Bardy, I, i. 2: “Hoc etiam ibi non tacui dicens. . . . Dixi quidem hoc, verumtamen paenitet me sic illic nominasse fortunam, cum videam homines habere in pessima consuetudine, ubi dici debet: ‘Hoc Deus voluit,’ dicere: ‘Hoc voluit fortuna,’” as quoted in Courcelle, *La “Consolation de Philosophie” dans la tradition littéraire*, 135. Although the point cannot be pursued here, it is worth noting that in his *Confessions* Augustine often seemed to interpret chance events, such as the death or conversion of a friend or a storm suffered by his mother on a sea voyage, as not only meaningful but specifically relevant to his personal perspective. He did not, therefore, “de-center” his reasoning to consider other possible interpretations or points of view.

because it happens to everyone without any explanation of merits, and it comes to good and bad alike.”<sup>56</sup>

In contrast to this earlier, disapproving tradition, twelfth-century poets and artists delighted in depicting fortune. An abbot of Fecamp had a mechanical wheel built “that his monks might ever have before them the spectacle of human vicissitudes,”<sup>57</sup> Andrew of Saint Victor in a letter to a fellow canon casually used the image of Fortuna’s wheel, which “raised me lately to a better state only to thrust me down to the bottommost depths of adversity.” Andrew lamented that “this brute . . . is allowed such boisterous rule, such power to rage against us.” By the late twelfth century Fortuna’s wheel was so common a figure that Richard FitzNigel could casually mention it in the *Dialogue of the Exchequer*.<sup>58</sup>

More significant than the popularity of Fortuna as a metaphor was its place in a trend toward seeing instruments other than the will of God as causative forces in history. Robert Hanning has shown that in the early twelfth century Anglo-Norman chroniclers began to use individual personalities and capacities as well as Fortuna to explain human events.<sup>59</sup> William of Malmesbury, for example, attributed the Norman conquests to the “prudence of William, seconded by the providence of God.”<sup>60</sup> In this new scheme of historical interpretation, Fortuna represented the chance and the capricious, distinct from both divine judgment and human intentions. Orderic Vitalis commented on the Norman Conquest that “changeable fortune often brings a hard and bitter fate to mortal men on earth, for some climb to the height of power, others are dashed from great prosperity to groan in the depths of despair.” In a similar vein, Henry of Huntington observed about William Rufus that “he formed mighty plans, which he would have brought to effect, could he have spun out the tissue of fate

<sup>56</sup> Isidore of Seville, *Etymologiaum sive originum*, bk. 8, 11, 94: “Fortuna a fortuitis nomen diciunt, quasi deam quandam res humanam variis casibus et fortuitis inludentum; unde et caecam appellant, eo quod passim in quolibet incurrens sine ullo examine meritorum, et ad bonos et ad malos venit.”

<sup>57</sup> *Bibliothèque de l’École des Chartres* (Paris, 1859), 154, as cited in Emile Mâle, *The Gothic Image: Religious Art in France of the Thirteenth Century*, trans. Dora Nussey (1913; reprint ed., New York, 1958), 94–97, 95 n. 4.

<sup>58</sup> Andrew of Saint Victor, Letter to Thomas, as quoted in Smalley, *The Study of the Bible in the Middle Ages*, 117; and C. Johnson, ed., *Dialogus de Scaccario* (London, 1950), 25–26. For a discussion of the attitudes of twelfth- and thirteenth-century philosophers toward the idea of fortune, see G. Paré, *Les idées et les lettres au XIII<sup>e</sup> siècle: Le Roman de la Rose* (Montreal, 1947), 124–31. Of particular interest is Aquinas’s definition of chance as the unforeseen intersection of two causal chains, since this conception is virtually identical to that of Cournot, which Piaget cited; compare Paré, *Les idées et les lettres au XIII<sup>e</sup> siècle*, 127, and Piaget and Inhelder, *The Origin of the Idea of Chance*, 1. Aquinas’s formal operational definition is quite unlike the concrete operational definition that Aristotle used; Aristotle contrasted chance occurrences and actions directed toward an end: “To say that chance is a thing contrary to rule is correct”; Aristotle, *Physics*, bk. 2, chap. 5. Aristotle also divided nature from chance: “Absence of chance and the serving of ends are found in the works of nature especially”; Aristotle, *On the Parts of Animals*, 645a. Similar comparisons can be made for definitions of speed, in which medieval science also marked a clear advance over ancient science; Piaget and Inhelder, *The Psychology of the Child*, 148n.

<sup>59</sup> Robert W. Hanning, *The Vision of History in Early Britain from Gildas to Geoffrey of Monmouth* (New York, 1966), 126: “The providential view of history was subtly modified in twelfth-century Norman historiography to allow a larger role for purely human causation, and to reflect a lively interest in psychological motivation; complementarily, divine providence was impersonalized to a certain extent, and even at times replaced by the concept of fortune’s ruling the affairs of men.”

<sup>60</sup> William of Malmesbury, *Gesta Regum Anglorum*, ed. William Stubbs, 2 vols. (London, 1887–89), bk. 3, 238, as quoted in Hanning, *Vision of History in Early Britain*, 226 n. 33.



or broken through and disengaged himself from the violence of fortune.”<sup>61</sup> This last remark is particularly striking, since any of a number of Rufus’s sins invited attributing his failures to divine punishment.

The naturalist philosopher Honorius Augustodunensis also mentioned Fortuna and her wheel in his *Speculum Ecclesiale*, but perhaps no twelfth-century appearance of Fortuna is more remarkable than her role in Alan of Lille’s *Anticlaudianus*. In this allegorical poem about the creation of the perfect man, “Fortune” is invited to bestow her gifts on the man after “Nature” and “the Virtues” have finished their work. Fortune, in answer, contrasts herself to nature while doubting that she has anything to add: “What will the god of chance be able to do where nothing is subject to chance? . . . Gold does not need iron nor light darkness. In the same way the act of Nature, the work of Virtue, the product of the heavenly Artisan, does not need my help and does not call for the services of my skill.”<sup>62</sup> To Alan of Lille, therefore, nature and fortune between them accounted for both the regular and the fortuitous, the predictable and the unexpected. But in the *Anticlaudianus* God remains a distant and shadowy figure—the creator and supreme arbiter, but not a participant in the activities of the poem. God’s direction of human activities seemed a little more remote at the end of the twelfth century than it had been at the beginning, and immanent justice appeared less of a ruling force than it had been in previous centuries.

The triumph of nature and fortune over earlier conceptions was far from complete by the end of the twelfth century. Jean de Meung, speaking significantly through the figure of nature, evidently had contemporaries in mind when he ridiculed those who, when viewing the destruction caused by storms,

. . . say the demons bring this all about  
With hooks and cables, or with teeth and nails;  
But such an explanation is not worth  
Two turnips; those accepting it are wrong,  
For nothing but the tempest and the wind  
Are needed to explain the havoc wrought;  
These are the things that cause the injury.<sup>63</sup>

The idea of immanent justice itself persisted for some time. Dante could speak of the ancient Romans having justified their world rule by ordeal. Oliver Cromwell read his victories as an indicator of God’s will. In 1871 Berlin celebrated Germany’s victory over France with a huge banner draped over the Brandenburg Gate bearing the inscription, “Durch Gottes Führung.” Even John D. Rockefeller could claim that God gave him his money. After 1100 it was thus up to the interpreter whether a given event was governed by God or Fortuna.

As long as there is no necessity that Roundhead and Cavalier, Frenchman

<sup>61</sup> Orderic Vitalis, *The Ecclesiastical History of Orderic Vitalis*, ed. and trans. Majorie Chibnall, 2 (Oxford, 1969): 179; and William of Malmesbury, *Gesta Regum Anglorum*, bk. 4, 333, as quoted in Hanning, *Vision of History in Early Britain*, 228 n. 51.

<sup>62</sup> Alan of Lille, *Anticlaudianus*, trans. James T. Sheridan (Toronto, 1973), 192. For a different approach to Fortuna, see Alexander Murray, *Reason and Society in the Middle Ages* (Oxford, 1978), 98–106.

<sup>63</sup> *The Romance of the Rose*, trans. Harry W. Robbins (New York, 1962), 381, ll. 24–30; for the French, see F. Lecoy, ed., *Le Romance de la Rose*, 3 (Paris, 1969): 37, ll. 17875–85.



and German, or capitalist and socialist agree, this ambiguity creates no difficulty. This was precisely the problem, however, that Alan of Lille's friend, Peter the Chanter, pointed to in his attack on ordeals. Now every verdict was subject to two interpretations. Granted that God could, if He wanted to, influence the outcome of ordeals, there was no certainty that He wanted to. In the absence of a miracle, hot iron would burn innocent and guilty alike, "from which I conclude the nature of these elements ought to be respected."<sup>64</sup> At each ordeal, therefore, the question could arise whether the judgment was God's or nature's. While tolerable in history or politics, such doubt has no place in a court of law. The intellectual changes of the twelfth century had stripped the ordeal of its usefulness.

HISTORIANS HAVE RECENTLY TRIED TO EXPLAIN medieval ordeals as a social or psychological adaptation to the trying circumstances of the early Middle Ages, so that a relaxation of those circumstances led naturally to the abolition of ordeals in the thirteenth century. But, since Piaget has found that children spontaneously believe in immanent justice and the perfect order of the universe, his work on causality, chance, and rituals helps place ordeals in the context of a logically structured conception of reality common in Europe between Augustine and Abelard. In this period most Europeans thought nature responded directly to God's will or the actions of other supernatural forces. Meaning could therefore be sought in the least occurrence, and rituals could be employed with the confidence that they controlled events. These beliefs about the world invited reliance on ordeals; indeed, if God's justice did not determine who sank in water or escaped maiming by the hot iron, how else would such results be explained? During the twelfth century precisely such explanations were suggested to intellectuals by their apprehension of natural cause and chance—sometimes a person might sink because he is heavy or survive burning because his skin was tough. In the face of these ideas, God's justice receded a bit from everyday life. Much that had seemed wondrous became, if not explained, at least explicable. Left more alone in the world, men had to find purely human means of working out their differences.

This account of the passage of the ordeal may be compared to what occurs in a scientific revolution. Thomas Kuhn has stressed that adopting a new paradigm in a scientific revolution involves more than a reinterpretation of familiar phenomena. Scientists must be open to possibilities they have not previously considered and must try to solve problems they have not previously conceived. They collect different kinds of data and often the data themselves change as the paradigms are altered. Thus, for example, Western astronomers first noticed sunspots, which the Chinese had been observing for centuries, only after the acceptance of Copernicus's picture of the solar system; and William Herschel's rec-

<sup>64</sup> Peter the Chanter, *Verbum Abbreviatum*, in *PL*, 205: col. 228A. For a later statement of this position, see Frederick Hohenstaufen, *The Liber Augustalis or Constitutions of Melfi Promulgated by the Emperor Frederick II for the Kingdom of Sicily in 1231*, trans. James M. Powell (Syracuse, 1971), 89-90 (bk. 2, title 31).

ognition that Uranus was a planet opened the way to the discovery of other orbiting bodies, the asteroids. "It is rather as if the professional community had been suddenly transported to another planet where familiar objects are seen in a different light and are joined by unfamiliar ones as well. . . . In so far as their only recourse to the world is through what they see and do, we may say that after a revolution scientists are responding to a different world."<sup>65</sup> Similarly, in the twelfth century intellectuals saw fire, rain, and lightning where their predecessors saw the miraculous intervention of God or the saints, and instead of devils they saw flies, wind, and chance misfortune. Reports of miracles were received with greater caution as natural explanations were sought out with increasing rigor. The impersonality of the universe was recognized, as was the futility of attempts to manipulate it by human rituals. Ordeals, which had fit so easily into the earlier scheme of things, had no place in the new natural order; regardless of their social utility, they had to go, just as phlogiston had to be banished from eighteenth-century chemistry.

Of course, the discovery of ordered nature differs from more usual scientific revolutions because it appears to have involved a shift not only in the categories but the kinds of logic applied to the physical world. And the assertion that twelfth-century attitudes were less primitive or more rational than earlier ideas may disturb those for whom these terms are value-laden adjectives and not objectively describable qualities. But twelfth-century philosophers of nature themselves asserted that the use of reason was what divided them from those who clung to older views. William of Conches assailed the laziness and ignorance of those who were content to assert that God made everything:

Ignorant themselves of the forces of nature and wanting to have company in their ignorance, they do not want people to look into anything; they want to believe like peasants and not to ask the reason behind things. . . . Although we know that most men are bent upon a splashy prose style and few of them have got hold of the truth, we ourselves, ignoring the multitude and glorying in the uprightness of those few, will sweat on after truth alone. We prefer to present the naked truth, not some dressed up falsehood. . . . Wretches! Could anything be more wretched than saying that something exists because God *can* make it, and yet not seeing that it does not exist, or having any reason why it should exist, or showing the usefulness for which it should exist? God does not do everything he is able to do.<sup>66</sup>

Adelard of Bath similarly exalted the power of reason to discover the laws of nature and condemned those who were content to accept old authorities instead of employing their own intelligence; he denounced those who did not try to appreciate the plan underlying the beauty of the universe as deserving to be cast out like someone who lived in a house and remained ignorant of how it was built.<sup>67</sup>

Approaching cultural change through the study of cognitive logic raises in a

<sup>65</sup> Kuhn, *The Structure of Scientific Revolutions* (Chicago, 1962), 114–18, 110. Kuhn acknowledged Piaget's influence on his own work; *ibid.*, viii.

<sup>66</sup> William of Conches, *Philosophia Mundi*, in *PL*, 172: 56–58, as quoted in Chenu, *Nature, Man, and Society in the Twelfth Century*, 11–12.

<sup>67</sup> Chenu, *Nature, Man, and Society in the Twelfth Century*, 11–12, 57. For a fuller development of this point, see Tina Stiefel, "The Heresy of Science: A Twelfth-Century Conceptual Revolution," *Isis*, 68 (1977): 347–63.

new and particularly acute way the problem of understanding culture and cognition. Until now, cross-cultural studies have tried to discover how culture affects cognition, either by promoting one set of specific beliefs instead of another or by producing differences in the rate at which children master certain basic logical skills. This consideration of ordeals suggests that influence may also run the other way, that cognition may affect culture. An individual's cognitive orientation mediates and often unconsciously changes doctrines received from others, as, for example, in Abelard's retention of Augustine's language while placing it in a novel logical structure. Although Piaget has suggested such a reversal before,<sup>68</sup> this complicated subject has scarcely been explored, and a few comments are appropriate here.

First, since cognitive skills vary considerably from person to person, it often happens that cultural change proceeds very unevenly, that people within the same society conceal under a shared vocabulary strikingly different ideas about the world. For example, Richard Kieckhefer has shown that in the late Middle Ages, uneducated accusers of witchcraft usually alleged that the supposed witch used incantations and charms without worrying much about how these rituals produced their effects. The reasoning underlying these beliefs seems close to that underlying the ordeal, and, in fact, "swimming a witch"—a variety of ordeal—remained a popular test of accused witches. Learned theorists, however, as heirs of the twelfth century "could not entertain seriously the notion that acts of sorcery and maleficent words or substances had inherent power to bring evil results without the mediation of demons."<sup>69</sup> Prosecutors thus made sense of allegations by transposing them to their own cognitive schema instead of by simply dismissing the accusations as irrational. Their example might serve as a warning to historians not to try too hard to find logical consistency in sources that appear to

<sup>68</sup> Jean Piaget, *Genetic Epistemology*, trans. E. Duckworth (New York, 1970), 6. According to Piaget, "The fundamental hypothesis of genetic epistemology is that there is a parallelism between the progress made in the logical and rational organization of knowledge and the corresponding formative psychological processes." He went on to suggest that "of course the most fruitful, most obvious field of study would be reconstituting human history—the history of human thinking in prehistoric man. Unfortunately, we are not very well informed about the psychology of Homo sapiens of Teilhard de Chardin." *Ibid.*, 13. The Middle Ages, by offering a possibly unique instance of the transformation of a primitive world-view in a literate society, may present an opportunity to test Piaget's hypothesis historically.

<sup>69</sup> Kieckhefer, *European Witch Trials: Their Foundation in Popular and Learned Culture, 1300–1500* (London, 1976), 79. As Kieckhefer has pointed out, "The motives for the transformation [of charges of sorcery to charges of diabolism] are clearer in the witchcraft treatises than in the court records. Essentially, the authors of this literature opposed a religious interpretation of sorcery to a magical one. They could not entertain seriously the notion that acts of sorcery and maleficent words or substances had inherent power to bring evil results, without mediation of demons. There was no place in their world-view for causation that was neither natural nor fully supernatural." Correspondingly, "there is no indication that the subtleties of causal connection had greatly concerned people in the lower strata of society; the repeated insistence in the witchcraft literature that curses and charms have no inherent power suggests that the authors were arguing against a deep-set popular conviction that such inherent power did obtain." *Ibid.*, 79, 84. Interestingly enough, Aquinas, in a discussion of witchcraft, insisted on the same distinction that Piaget made between words as symbols and as things efficacious in themselves. "Now, words, in so far as they signify something, have no power except as derived from the intellect either of the speaker, or of the person to whom they are spoken. . . . Now it cannot be said that these significant words uttered by magicians derive efficacy from the intellect of the speaker. . . . Moreover, man's intellect is invariably of such a disposition that its knowledge is caused by things, rather than it is able by its mere thought to cause things." Aquinas, *Summa contra Gentiles*, bk. 3, pt. 2, chap. 105, as quoted in A. C. Kors and Edward Peters, *Witchcraft in Europe, 1100–1700* (Philadelphia, 1972), 57. And see Piaget, *The Child's Conception of the World*, 162.

be speaking nonsense. Sometimes nonsense really is nonsense, at least when measured against different conceptions of the world.

Second, it is likely that many primitive cultures are shaped by “egocentric” intellectual concerns. During the early Middle Ages, Christian cosmology was employed to offer explanations of everyday phenomena and to provide a rationale for rituals like ordeals and maledictions. The gods in the Homeric poems serve a similar function: Paris’s helmet strap does not break by chance during his duel with Menelaos; Aphrodite broke it for her own purposes. For many primitive peoples, magic and witchcraft today still lend meaning to the world. E. E. Evans-Pritchard, for example, has shown how the Zande explain by witchcraft events that modern man would simply ascribe to chance; he has also noted how they combine natural and supernatural causation rather like Piaget’s children and the medieval litigants who trusted in God and employed skilled champions to fight their judicial duels.<sup>70</sup> Quite possibly, the pervasiveness and tenacity of such beliefs stem, in part, from the persuasiveness of the ideas themselves. Piaget, for example, offered the hypothesis that there is “continuity between the purely individual idea of efficacy and the idea implied in the social beliefs of a magical type. This does not in the least suggest that the social beliefs have not—precisely because they are social—an infinitely greater power of coercion and crystallization. It means simply that they are made possible by means of an individual psychological substructure.”<sup>71</sup> Piaget’s hint appears to have received very little attention, but the instance of the ordeal—itself apparently a result of this process—suggests that further comparative study may be merited.

If a comparison of early medieval culture to those of primitive societies is valid, then twelfth-century Europe merits attention as a society that became critical of its magical beliefs and developed scientific conceptions. As long as divine justice and supernatural forces were thought to inspire everyday events, a scientific attitude was impossible. Augustine, for example, had little use for science: at best it was trivial, at worst pointless, since the laws of nature reveal not physical necessity but God’s will.<sup>72</sup> Once men perceived nature as a system, however, they could and did resume scientific investigation. In the first decades of the twelfth century Adelard of Bath contributed both translations of Arabic works and original treatises to Latin scientific literature. His lead was followed by many others in the twelfth century, and by the thirteenth century scientists were advancing beyond their Greek predecessors. Instances of this kind of shift appear to be rare; classical Greece is the most obvious other case (for China, for example, never achieved the notion of natural law).

Until more is known about the relationship between cognition and culture, defining with much precision the causes of the shift in the idea of nature will be difficult. It does, however, seem clear that many traditional explanations of cultural change—reception of new ideas, growth of education, class interest—need

<sup>70</sup> Evans-Pritchard, *Witchcraft, Oracles, and Magic among the Azande* (Oxford, 1937), 63–68.

<sup>71</sup> Piaget, *The Child’s Conception of the World*, 392.

<sup>72</sup> Robert M. Grant, *Miracle and Natural Law in Graeco-Roman and Early Christian Thought* (Amsterdam, 1952), 217.

to be revised to give more recognition to circumstances that inhibit or accelerate the breakdown of cognitive egocentricity. For example, Benjamin Nelson, following Weber, has argued that science was a way of reconciling discrepant points of view that were brought into contact by increased communication and the growth of towns.<sup>73</sup> Such an interpretation in effect presents at the sociological level the process that Piaget described as de-centering at the psychological level. By stressing experience and the interaction of individuals with their environment, this hypothesis avoids the dangers inherent in any simple equation of social organization and individual beliefs. Neither Weber nor Nelson, however, has tried to describe this process in any detail, and careful research in this direction may prove fruitful.

These theoretical considerations appear to take us well beyond the initial problem of understanding ordeals, but then ordeals present a complex interaction between beliefs and institutions. The ordeal's efficacy at resolving disputes and the ability of its judgments to command respect depended not on courts of law but on a set of beliefs about the world. When those beliefs disappeared, institutions had to adopt new forms of proof or forfeit their claim to dispense justice. Sometimes, at least, people do shape institutions; it is not always the other way around.

<sup>73</sup> Nelson, "Sciences and Civilizations, 'East' and 'West': Joseph Needham and Max Weber," in Raymond J. Seeger and Robert S. Cohen, eds., *Boston Studies in the Philosophy of Science*, no. 11 (Boston, 1974), 445-93.

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## *AHR Forum*

### Class Structure and Economic Development in the American South, 1865–1955

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HISTORIANS HAVE LONG REGARDED the postwar American South as a society of poverty and racial oppression, but, with some outstanding exceptions, few have attempted to analyze its economic and social development in a systematic and comprehensive way. Several recent publications indicate that this period of neglect has come to an end. Inevitably, the intense debate over the slave South has raised questions about the relationship of slavery to postwar development; it has also presented historians of the postwar period with well-developed alternative theories that help provide answers. Among studies of the antebellum South, those of Eugene D. Genovese and of Robert W. Fogel and Stanley L. Engerman stand out for their self-conscious theoretical clarity. The new works on postwar development similarly divide into two basic theoretical approaches: those historians who follow Fogel and Engerman apply neoclassical economic theory and analyze Southern development according to the laws of the market; those who follow Genovese and C. Vann Woodward analyze the South in terms of its constituent classes and see its development as the outcome of conflicts among them. Although these schools by no means exhaust the ways of viewing the problem, they represent much of the important new work. Of the two, class analysis, particularly as represented by Harold D. Woodman's recent studies, provides a more satisfactory basis for understanding the postwar South than neoclassical economics. What is lacking in the existing debate, however, is an appreciation of the labor-repressive character of class relations in the postwar South, which made its development qualitatively different from that of the North.

THE NEOCLASSICAL ECONOMIC HISTORIANS have applied the theory of *Time on the Cross* to the postwar South. Joseph D. Reid, Jr., Stephen J. DeCanio, and Robert Higgs have argued that the region's socioeconomic institutions arose in the

For their encouragement and criticism, I thank Robert Brenner, John P. Diggins, Eugene D. Genovese, Thomas L. Haskell, James Henretta, Michael P. Johnson, Temma Kaplan, Michael Reich, Armstead Robinson, Gavin Wright, and Maurice Zeitlin.



postwar period as a result of the rational functioning of a free market system, in which individuals freely made choices about exchanging different, relatively scarce “factors” and in which all were treated as equals. The economy developed more or less automatically in response to the “laws of supply and demand”; the resulting institutions were rational and efficient. Of course, racial prejudice did exist, but it primarily affected political rather than economic relations; the free market defeated efforts of white racists to interfere with its relentless logic. While these historians have claimed that the relevance of racism is confined to politics, they have denied the relevance of social classes altogether.

In Reid’s view, sharecropping arose as a rational system of increasing agricultural productivity by minimizing transaction costs, distributing risk, and bringing about an “optimum distribution of management talent among tenants”—that is, white management of black tenants.<sup>1</sup> Reid has presented this analysis entirely in the form of deductive theorizing, without providing empirical data to support it. Unlike Reid, DeCanio has been concerned with evidence. Devoting more than one hundred pages at the outset of his *Agriculture in the Postbellum South* to presenting—and then dismissing—“impressionistic” evidence, he has argued that historians should disregard nonquantitative evidence because it consists of conflicting statements. Did planters use violence to intimidate the freedmen? Carl Schurz said yes, a Tennessee planter said no; DeCanio declared that in such cases both sides are “equally persuasive” and has concluded that “non-quantitative tests simply do not have the power to resolve historical reality.”<sup>2</sup> He has thus dismissed the efforts of historians to evaluate different sources of evidence in terms of reliability, relevance, and adequacy. But, if anything has been proved by the tidal wave of criticism that engulfed Fogel and Engerman’s *Time on the Cross*, it is that quantification alone does not “resolve historical reality” or eliminate conflicting interpretations, even among quantifiers.

DeCanio has presented high-powered, narrow, quantitative evidence of the rationality of the Southern economy’s response to market forces. Using fifty-five equations on “theoretical issues” and a table of production function estimates nineteen pages in length, he has concluded that Southern sharecroppers were not exploited; on the contrary, labor received “if anything . . . a wage somewhat higher than the value of its marginal product.” Southern farmers planted cotton and corn in rational proportions as market conditions changed; whatever poverty and oppression blacks suffered was the consequence of their “non-human factor endowment,” an expressive phrase for their lack of land and capital.<sup>3</sup> But

<sup>1</sup> Reid, “Sharecropping as a Understandable Market Response: The Post-Bellum South,” *Journal of Economic History*, 33 (1973): 127. Reid has also written that the Civil War and Reconstruction “continue to defy historical understanding”; Reid, “Understanding Political Events in the New Economic History,” *ibid.*, 37 (1977): 307.

<sup>2</sup> DeCanio, *Agriculture in the Postbellum South: The Economics of Production and Supply* (Cambridge, Mass., 1975), 25, 53, 51, 12.

<sup>3</sup> *Ibid.*, 12, 14. DeCanio has not indicated with whom he was taking issue on the question of labor’s marginal product, although he has admitted that his evidence does not challenge the Marxist view; *ibid.*, 4. Nor does his evidence contravene that of Roger Ransom and Richard Sutch, who have shown that tenants were exploited in the credit market; see Ransom and Sutch, *One Kind of Freedom: The Economic Consequences of Emancipation* (New York, 1977), 162–70. For a more satisfactory specification of the general problem of Southern eco-

DeCanio has shown little interest in trying to understand either the origins or the consequences for the Southern economy of blacks' lack of land and capital. In narrowing both the questions and the evidence, his work has limited value for historians concerned with the broader questions of the pace of Southern economic development and the sources of the region's characteristic poverty and oppression.

Higgs's work, written for the nonspecialist, is more concerned with these larger questions. Like Reid, Higgs has emphasized the economic rationality of a system in which landlords were white and tenants were black, because this division permitted blacks to "serve . . . apprenticeships" and gain "valuable experience . . . cultivating cotton and corn." The high interest rates charged blacks were also rational in view of the former slaves' "poor reputation as borrowers." But, unlike Reid, Higgs has sought to show that the postwar Southern economy facilitated a "rapid growth of black incomes" and "major improvements" in the standard of living. He calculated that black per capita income more than doubled in the last third of the century, growing one-third faster than the American economy as a whole.<sup>4</sup> But this rate of growth is misleading; given the socioeconomic conditions of blacks just after the Civil War, the "nadir of southern economic history," any subsequent comparison would suggest an impressive rate.<sup>5</sup> Higgs's absolute figures show that annual black per capita income in 1900 was forty-three dollars higher than it had been in 1867 and that the average white in 1900 earned three times more than the average black. Higgs could thus "marvel that the freedmen did so well" and attribute their "success" to their "learning experiences" as sharecroppers under white landlords<sup>6</sup>—a questionable judgment and an unconvincing explanation.

Reid, DeCanio, and Higgs have shared an overriding concern: each has wanted to show that the economy was not responsible for black poverty and oppression in the postwar South. For them, the economy provided a realm of equality, freedom, and achievement that blacks were denied in political and social life. None of them has gone beyond this position to examine the sources of the blacks' low standard of living or to explain the slow economic development of the region, except to refer briefly to noneconomic causes. DeCanio merely noted that "various repressive laws and acts of violence perpetrated against

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economic development, see Stanley Engerman, "Some Factors in Southern Backwardness in the Nineteenth Century," in John F. Kain and John R. Meyer, eds., *Essays in Regional Economics* (Cambridge, Mass., 1971), 279–306.

<sup>4</sup> Higgs, *Competition and Coercion: Blacks in the American Economy, 1865–1914*, Hoover Institution Publication, no. P 163 (New York, 1977), 121, 72, 102, 117.

<sup>5</sup> Gavin Wright, *The Political Economy of the Cotton South: Households, Markets, and Wealth in the Nineteenth Century* (New York, 1978), 159.

<sup>6</sup> Higgs, *Competition and Coercion: Blacks in the American Economy*, 102, 121. The case of Nate Shaw, a black tenant farmer in early twentieth-century Alabama, suggests an alternative explanation. Shaw set out to farm on his own to disprove the notion that blacks required white supervision to work efficiently. He never completed the test, however, because a sheriff's posse drove him off his land, shot him in the back, and jailed him for twelve years. Shaw concluded that the landlord class used its political power to keep blacks as sharecroppers—that force, rather than rational free choice, lay at the base of the South's economy and society. See Theodore Rosengarten, *All God's Dangers: The Life of Nate Shaw* (New York, 1974); and my Review Essay of Rosengarten's *All God's Dangers* in the *Journal of Social History*, 10 (1976): 170–78.

blacks were indeed widespread but were not instruments of economic exploitation in the labor market"; Higgs allowed that obstacles to black achievement lay in a "taste for . . . racial discrimination" among whites, expressed primarily in politics, but argued that "discrimination fail[ed] completely" when it came up against the competitive laws of the market economy. "Neither white benevolence nor black power had much to do with the attainment of these non-discriminatory outcomes," he insisted,<sup>7</sup> adopting a position that can only be regarded as the ultimate form of economic determinism.

WHY WOULD THESE ECONOMIC HISTORIANS take such a position? Despite the scientific appearance of their work, they have an ideological commitment to the notion that competitive capitalism, if left alone, maximizes benefits for everyone in society—that the problems of the South in general and of the blacks in particular resulted from interference with the market. The political implication is that government intervention reduced the capacity of the economy to bring progress and prosperity to blacks.

Reid, DeCanio, and Higgs have seen decentralized family sharecropping as the most rational organization of agriculture, not only for blacks but also for white planters. The social history of the immediate postwar period, however, indicates that the planters reached different conclusions about what constituted economic rationality. They believed that the most profitable course was to maintain the plantation as a centralized unit of production. By using supervised gang laborers who were paid wages and incorporated into the organizational structure of the antebellum plantation, the planters hoped to preserve economies of scale and centralized management. Engerman has argued that this program was fully comprehensible as an effort to maintain the method and social organization of production developed over sixty years; his study of land values and of output per worker indicates that "the planters were correct in the attempts to return to gang labor."<sup>8</sup>

The three cliometricians have exaggerated the extent to which landlords freely competed for tenants. Planters organized to limit the free market in labor and to force freedmen to work on plantation gangs, sought to enlist the Freedmen's Bureau in the same effort, and worked in the state legislatures to establish repressive laws. Some turned to terror—to the Ku Klux Klan—to force blacks to labor in plantation gangs. Planters throughout the South in the years immediately following the war organized to limit competition among themselves. At a typical meeting in the fall of 1867, planters in Sumter County, Alabama, unanimously resolved that "concert of action" was "indispensable" in hiring la-

<sup>7</sup> DeCanio, *Agriculture in the Postbellum South*, 13; and Higgs, *Competition and Coercion: Blacks in the American Economy*, 130, 131.

<sup>8</sup> Stanley Engerman, "The Legacy of Slavery," paper delivered at the Duke University Symposium on *One Kind of Freedom*, Durham, N.C., February 11, 1978, p. 14. Ransom and Sutch have taken issue with this interpretation, as has Gavin Wright; see Ransom and Sutch, *One Kind of Freedom: The Economic Consequences of Emancipation*, 70–77; and Wright, *Political Economy of the Cotton South*, 74–87.

bor. Thus, all would offer the same terms to the freedmen, and none would “employ any laborer discharged for violation of contracts.” Other planters held similar meetings in places like Sumter, South Carolina, and Amite County, Mississippi, followed by state-wide meetings of planter representatives. The report of the Freedmen’s Bureau in 1866 complained of the planters’ “community of action,” and the Joint Congressional Committee on Reconstruction heard evidence on the same phenomenon.<sup>9</sup> As one planter explained the strategy to John Trowbridge in 1866, “The nigger is going to be made a serf, sure as you live. It won’t need any law for that. Planters will have an understanding among themselves: ‘You won’t hire my niggers, and I won’t hire yours,’ then what’s left for them? They’re attached to the soil, and we’re as much their masters as ever.”<sup>10</sup> Planters went beyond these informal organizations and used state power to enforce the interests of their class and prevent those individualists among them who desired to engage in market economics. “Enticement” acts passed in every Southern state immediately after the war made it a crime to “hire away, or induce to leave the service of another,” any laborer “by offering higher wages or in any other way whatsoever.”<sup>11</sup> The criminal defined by this law was not the black who left his plantation, but the planter who sought a free market in labor.

Reid, DeCanio, and Higgs have also exaggerated the extent to which blacks were free to move. Louisiana law, for instance, made it a crime to “feed, harbor, or secrete any person who leaves his or her employer,” and enticement laws in most states provided that farm laborers hired away by better offers could be forcibly returned to the original employers. Vagrancy acts were even more extreme efforts to control the mobility of labor. The definition of vagrancy usually included “stubborn servants . . . , a laborer or servant who loiters away his time, or refuses to comply with any contract . . . without just cause.”<sup>12</sup> Planters could thus enlist local courts in keeping “their” laborers on their plantations.

The planters’ bitter opposition to the presence of the Freedmen’s Bureau did not stop them from seeking to enlist the bureau’s agents in an effort to tie blacks to the land. Planters put intense and calculated social pressure on the Union representatives in their midst. As early as 1864, a War Department report warned that officials in charge of the freedmen were “received into the houses of the planters and treated with a certain consideration,” so that, under the “influences” that the planters brought to bear, officials often (“without becoming fully

<sup>9</sup> Montgomery *Daily Advertiser*, October 31, 1867; Joel Williamson, *After Slavery: The Negro in South Carolina during Reconstruction, 1861–1877* (Chapel Hill, 1965), 99; James L. Roark, *Masters without Slaves: Southern Planters in Civil War and Reconstruction* (New York, 1977), 135; “Report of the Commissioner of the Bureau of Refugees, Freedmen, and Abandoned Lands,” Papers Accompanying the Report of the Secretary of War, House Exec. Doc. no. 1, 39th Congress, 2d Sess., 3 (Washington, 1866): 706; and “Report of the Joint Committee on Reconstruction,” House Report no. 30, 39th Congress, 1st Sess., 2 (Washington, 1866): 9.

<sup>10</sup> John Townsend Trowbridge, *The South: A Tour of Its Battle-Fields and Ruined Cities* . . . (Hartford, 1866), 427. Higgs, though aware of these efforts to organize, has asserted that they failed because they were irrational for the individual planter seeking labor in the free market; Higgs, *Competition and Coercion: Blacks in the American Economy*, 47–48.

<sup>11</sup> *Acts of the Session of 1865–6, of the General Assembly of Alabama* (Montgomery, 1866), 111–12; and *Acts of the General Assembly of the State of Georgia* . . . [1865–66] (Milledgeville, Ga., 1866), 153–54.

<sup>12</sup> *Acts Passed by the General Assembly of the State of Louisiana, at the Extra Session . . . November 1865* (New Orleans, 1866), 24–26; and *Acts of the Session of 1865–6, of the General Assembly of Alabama*, 119–21.

conscious of it") became "the employers' instrument of great injustice and ill-treatment toward . . . colored laborers."<sup>13</sup> A black-belt newspaper explained in 1866 that Union officials who were "gentlemen" were "received into the best families . . . on probation" but those who kept "company with Negroes . . . could not get into society."<sup>14</sup> When the Freedmen's Bureau opened an office in the Alabama black belt, "the white people . . . determined to win their good will," according to Walter L. Fleming. "There were 'stag' dinners and feasts, and the eternal friendship of the officers, with a few exceptions, was won."<sup>15</sup> Fleming gave more credit to the persuasive power of stag dinners with planters than the feasts probably deserved, but he was undoubtedly correct in describing the planters' intentions as well as the effect of their efforts: some agents of the Freedmen's Bureau helped planters get freedmen to work on terms agreed to by planter organizations and often sided with planters in disputes with freedmen.

Finally, some planters restricted blacks' freedom to move by resorting to terror. Historians concerned with the politics of Reconstruction have overlooked the extent to which the Klan in the black belt was an instrument of the planter class for the control of labor. Planters played a major role in organizing and directing Klan activities there; and Klan terror contributed to the repression of black labor, primarily by threatening those who contemplated emigration. As early as 1866, masked bands "punished Negroes whose landlords had complained of them."<sup>16</sup> According to the Congressional testimony of one planter, when blacks "got together once to emigrate . . . , disguised men went to them and told them that if they undertook it they would be killed," in order to keep "the country from being deprived of their labor." In the words of a black belt lawyer, the Klan was "intended principally for the negroes who failed to work."<sup>17</sup> And Allen W. Trelease has shown that the Klan pursued blacks who "violat[ed] . . . labor contracts by running away."<sup>18</sup>

Despite the planters' use of informal organization, formal law, the Freedmen's Bureau, and the Klan, they failed to preserve the plantation as a centralized unit cultivated by gangs of wage laborers. To understand this failure, it is necessary to look beyond the abstract logic of the market and focus on the relatively concrete process of class conflict between planters and freedmen. Rational as the planters' effort was, preservation of the centralized plantation confronted an insurmountable obstacle: the freedmen's refusal to agree to it. Their widespread resistance to working for wages in gangs, which appears in the sources as a "shortage of labor," played a crucial role in the reorganization of agriculture after the war. Such shortages were reported throughout the plantation South in

<sup>13</sup> James McKaye, *The Mastership and Its Fruits—The Emancipated Slave Face to Face with His Old Master: A Supplementary Report to Hon. Edwin M. Stanton, Secretary of War* (New York, 1864), reprinted in the *National Anti-Slavery Standard*, July 23, 1866.

<sup>14</sup> *Montgomery Weekly Mail*, April 26, 1866.

<sup>15</sup> Fleming, *Civil War and Reconstruction in Alabama* (Cleveland, 1911), 449–50. Also see William S. McFeely, *Yankee Stepfather: General O. O. Howard and the Freedmen* (New Haven, 1968), 158–59.

<sup>16</sup> Allen W. Trelease, *White Terror: The Ku Klux Conspiracy and Southern Reconstruction* (New York, 1971), 81.

<sup>17</sup> "Testimony Taken by the Joint Select Committee to Inquire into the Condition of Affairs in the Late Insurrectionary States," House Report no. 22, pt. 5, 42d Congress, 2d Sess., 3 (Washington, 1872): 1466, 487.

<sup>18</sup> Trelease, *White Terror*, 290.



the immediate postwar years. Robert Somers, who visited the South in 1871 and wrote a book about his experiences, titled his chapter on the Alabama black belt "Despair of the Planters for Labor." Reports by the Freedmen's Bureau and the Boston textile firm of Loring and Atkinson concurred.<sup>19</sup> The freedmen's idea of a rational system of production differed from that of the planters; the blacks hungered for land. Eugene Genovese has quoted a plantation mistress's description of a typical situation at the war's end: "our most trusted servant . . . claims the plantation as his own."<sup>20</sup> The Joint Congressional Committee on Reconstruction noted the freedmen's fierce "passion . . . to own land," and the *Montgomery Advertiser* agreed that blacks were "ravenous for land." The freedmen made their claim on the basis of a kind of labor theory of value; as a "Colored Convention" proclaimed in Montgomery in May 1867, "the property" that the planters held was "nearly all earned by the sweat of our brows, not theirs."<sup>21</sup> And an exslave wrote in 1864, "we wants land—dis bery land dat is rich wid de sweat ob we face and de blood ob we back."<sup>22</sup>

By creating a "shortage of labor," the freedmen defeated the planters' efforts to preserve the plantation as a single, large-scale unit worked by gangs. Increasingly in 1867 and 1868, planters divided their plantations into small plots and assigned each to a single family. In establishing decentralized family sharecropping as the prevailing organization of cotton production after the war, the planters made a major concession to the freedmen and their resistance to the slavelike gang system. The Selma *Southern Argus*, one of the most articulate and insightful voices of the planter class, admitted this explicitly: sharecropping was "an unwilling concession to the freedman's desire to become a proprietor . . . , not a voluntary association from similarity of aims and interests."<sup>23</sup> Thus, class conflict shaped the form of the postwar plantation more than did purely economic forces operating according to the logic of the free market.<sup>24</sup>

<sup>19</sup> Somers, *The Southern States since the War, 1870-1* (New York, 1871), 165; "Report of the Commissioner of the Bureau of Refugees, Freedmen, and Abandoned Lands," 706; and F. W. Loring and C. F. Atkinson, *Cotton Culture and the South Considered with Reference to Emigration* (Boston, 1869).

<sup>20</sup> Kate Stone, *Brokenburn*, ed. J. Q. Anderson (Baton Rouge, 1955), 193, as quoted in Genovese, *Roll, Jordan, Roll: The World the Slaves Made* (New York, 1974), 108.

<sup>21</sup> "Report of the Joint Committee on Reconstruction," 36; *Montgomery Daily Advertiser*, January 17, 1883; and *Montgomery Daily State Sentinel*, May 21, 1867.

<sup>22</sup> Letter from Uncle Smart (n.d.), in the *Philadelphia Press*, May 31, 1864, as reprinted in James M. McPherson, *The Negro's Civil War: How American Negroes Felt and Acted during the War for the Union* (New York, 1965), 297. For planter resistance on this issue, see Michael Perman, *Reunion without Compromise: The South and Reconstruction, 1865-1868* (New York, 1973), 39; and Roark, *Masters Without Slaves*, 142. Reid and Higgs have recognized the planters' desire to organize the postwar plantation on the basis of gang labor, but neither of them has been able to account for the failure of this effort. Higgs has vaguely suggested that "certain problems inherent in the wage system" made it "unsatisfactory to both employers and employees," while Reid, in a similar vein, has stated that planters "seemingly felt the work gang system more conducive to effective direction of the labor force, but apparently not worth the requisite wage premium"; see Higgs, *Competition and Coercion: Blacks in the American Economy*, 49, 45; and Reid, "Sharecropping as an Understandable Market Response," 111.

<sup>23</sup> *Selma Southern Argus*, March 17, 1870.

<sup>24</sup> Higgs and DeCanio have discussed the planters' effort to reduce black mobility but have argued that such attempts failed. Neither has explained the process by which sharecropping rather than gang labor came to prevail. Reid's and Higgs's analyses of the prevalence of different forms of tenancy at different times are unconvincing, as Gavin Wright has demonstrated. Rather than regarding these differences as a consequence of rational decisions about risk-sharing, considering them primarily as a consequence of a tenant's assets seems more plausible. One who possesses nothing but labor power can only be a sharecropper; one with a mule and



THE LEADING ALTERNATIVE ANALYSIS of the development of the postwar South rejects neoclassical free market economic theory, taking historically concrete social classes, rather than abstractly rational individuals, as the starting point. Harold D. Woodman has recently applied this theory to postwar Southern development in a powerful and persuasive way. Following the interpretation C. Vann Woodward developed in his *Origins of the New South*, Woodman has combined a level of theoretical clarity rare among historians with a rich knowledge of history seldom found among economists. His penetrating critical studies of neoclassical economic history are unsurpassed. Woodman has emphasized the dynamic quality of the postwar Southern transformation: the defeat of the Confederacy started the South off on the same path that the North had followed—the development of capitalist class relations, “the making of a working class from former slaves” and of a genuine bourgeoisie from former masters. In the process, the plantation became a “thoroughly capitalistic farm.”<sup>25</sup> Postwar Southern society was evolving in the same direction as the rest of the nation, though at a slower pace because of the ideological and cultural heritage of slavery. Particularly significant was the planters’ racism, as was their continued antagonism to the bourgeois notions of equality and freedom—the bases of the capitalist concept of free labor and of the concomitant view of the wage contract as an agreement between equals. This ideological heritage, Woodman has contended, did not make the region’s socioeconomic development different in kind from that of the North; the South, too, was an “evolving bourgeois society.”<sup>26</sup>

Roger Ransom and Richard Sutch have occupied a middle ground among students of Southern development. Starting from neoclassical market theory, they have come to conclusions closer to those of Woodman and Woodward than to those of Reid, DeCanio, and Higgs: racism slowed capitalist development in the postwar South almost to a halt. Like Woodman, Ransom and Sutch have not seen Southern development as a distinct type; rather, it was a version of Northern development, distinguished by its slower pace. “Racism distorted the economic institutions of the South,” Ransom and Sutch have stated, “reshaping them so that the market signals—which normally direct resources toward their most productive employment and provide the incentive to the investments and the innovations that propel economic growth—were either not generated or were greatly weakened.”<sup>27</sup> Ransom and Sutch have discussed the critical ques-

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tools can be a cash renter. See Reid, “Sharecropping in History and Theory,” *Agricultural History*, 49 (1975): 426–40; Higgs, “Patterns of Farm Rental in the Georgia Cotton Belt, 1800–1900,” *Journal of Economic History*, 34 (1974): 468–82, and “Race, Tenure, and Resource Allocation in Southern Agriculture, 1910,” *ibid.*, 33 (1973): 151–59; DeCanio, *Agriculture in the Postbellum South*, 51–76; and Wright, *Political Economy of the Cotton South*, 176.

<sup>25</sup> Woodman, “Sequel to Slavery: The New History Views the Postbellum South,” *Journal of Southern History*, 43 (1977): 552–53; and Woodward, *Origins of the New South, 1877–1913* (Baton Rouge, 1951). On the social and economic bases of the Confederacy, see Michael P. Johnson, *Towards a Patriarchal Republic: The Secession of Georgia* (Baton Rouge, 1977); and Raimondo Luraghi, *The Rise and Fall of the Plantation South* (New York, 1978), chaps. 8–10.

<sup>26</sup> Woodman, “Sequel to Slavery,” 554.

<sup>27</sup> Ransom and Sutch, *One Kind of Freedom: The Economic Consequences of Emancipation*, 177. Their most valuable contribution has been to show that destruction during the Civil War does not necessarily explain the slow

tion of the relationship of Southern to Northern development. "The large gaps between southern and northern incomes should have attracted southern labor to the North," they observed; that it did not requires explanation. Although elsewhere they have recognized the existence of peonage, in this discussion they pointed to two factors: illiteracy (it was "difficult for black workers to learn about economic opportunities" in other parts of the country) and poverty ("migration was expensive").<sup>28</sup> Lack of education and lack of income, in turn, were consequences of the racist prejudices of Southern whites.

From the perspective of class analysis, blacks suffered the greatest exploitation and oppression, but the system of labor restrictions centering on debt peonage—requiring an individual to labor against his will to satisfy a debt—expanded to include whites as well. The system was, therefore, not exclusively racial in orientation—all whites dominating all blacks—but a class system—white planters dominating tenants of both races, with blacks forming the most oppressed part of the working class.

PROPOSERS OF BOTH INTERPRETATIONS—neoclassical economic history and the "evolving bourgeois society" approach—have minimized the distinction between Southern sharecropping and Northern wage labor. The North's economy depended on the market mechanism to allocate "free" labor; capitalists competed for labor and laborers were free, at least in theory, to move in response to better offers. This was the "classic capitalist" route to industrial society. Until the Great Depression of the 1930s, planters in the postwar South used more directly coercive methods of labor allocation and control. These restrictions on the South's labor market distinguished the planter from the Northern bourgeois, turned the sharecropper into a kind of "bound" laborer, and made the development of postwar Southern capitalism qualitatively different from the Northern pattern.

To argue that Northern agricultural laborers enjoyed freedoms denied to their Southern counterparts is not to say that the capitalist development of the North eliminated exploitation, oppression, or poverty. But their characteristic forms were different in kind from those under which Southern sharecroppers labored. The typical laborer in the "bonanza" wheat farms of the Northern plains was a migrant wage worker who was oppressed not by peonage but by seasonal unemployment and the need to travel great distances over the course of the year. The terms of disparagement for these workers—"tramps," "bums," "riff-raff"—precisely described their mobility, their absence of ties to the land. In other areas of the North, agricultural laborers worked primarily as "hired hands" on family farms and received wages by the month or, during the harvest

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pace of Southern development; *ibid.*, 40–55. In addition, they have accounted for the origins of sharecropping in terms of conflict rather than free market rationality; *ibid.*, 81–103. For a similar analysis, see Thomas Holt, *Black over White: Negro Political Leadership in South Carolina during Reconstruction* (Urbana, 1977), 25, 68–69.

<sup>28</sup> Ransom and Sutch, *One Kind of Freedom: The Economic Consequences of Emancipation*, 193, 195.

season, by the day. Farm labor took other forms in the truck gardens of the East and on the great farms of California's central valley. Studies of tenancy in the Midwest contain no evidence that debt peonage was widespread.<sup>29</sup> Northern farm laborers were "forced to be free," the fate of labor wherever agriculture develops in classic capitalist fashion.

The most important institution in the South's system of bound labor was debt peonage. Pete Daniel's work on this central element in postwar Southern history is indispensable.<sup>30</sup> Tenants began each season unable to finance the year's crop and had to seek credit from their landlords or the local merchants, who required that the tenant remain until the debt was paid, however many seasons that took. Hard-working tenants could be made to stay by exaggerating their indebtedness through dishonest bookkeeping; undesirable ones could be ordered to move on, with their debts transferred to a new landlord. The movement of tenants among landlords preserved the system's repressive nature as long as the debt moved with the tenant, as typically it did. Movement alone does not, therefore, disprove the existence of debt peonage. Its extent is difficult to measure precisely; no doubt it varied along with economic cycles. Most contemporaries and historians describe it as a characteristic feature of cotton agriculture in the postwar South, and one study has found that 80 percent of the sharecroppers in Alabama had an indebtedness of more than one year's standing.<sup>31</sup>

<sup>29</sup> Of course, Northern capitalists enlisted the state in repressing labor, with great violence on occasion, but in a qualitatively different way from that prevalent in the South. For a work of fundamental importance on agricultural labor, see LaWanda F. Cox, "The American Agricultural Wage Earner, 1865-1900: The Emergence of a Modern Labor Problem," *Agricultural History*, 22 (1949): 94-114, esp. 103-06. Northern tenants feared not peonage but eviction; see Paul W. Gates, "Frontier Estate Builders and Farm Laborers," in Walker D. Wyman and Clifton B. Kroeber, eds., *The Frontier in Perspective* (Madison, Wisc., 1957), 143-64; Margaret B. Bogue, *Patterns from the Sod: Land Use and Tenure in the Grand Prairie, 1850-1900* (Springfield, Ill., 1959), 156-75; and Allen G. Bogue, *From Prairie to Corn Belt: Farming on the Illinois and Iowa Prairies in the Nineteenth-Century* (Chicago, 1963), 182-84. For a Higgsian interpretation, see Donald L. Winters, "Tenancy as an Economic Institution: The Growth and Distribution of Agricultural Tenancy in Iowa, 1850-1900," *Journal of Economic History*, 37 (1977): 382-408.

<sup>30</sup> Daniel, *The Shadow of Slavery: Peonage in the South, 1901-1969* (Urbana, 1972). Also see Michael Schwartz, *Radical Protest and Social Structure: The Southern Farmers' Alliance and Cotton Tenancy* (New York, 1976), chap. 6. Unlike Higgs and Reid, Gavin Wright has argued that debt peonage contributed to the irrationality of the Southern economy; the only way for a sharecropper to escape debt peonage was to abandon self-sufficiency and concentrate on growing the cash crop—cotton. But, because world prices were falling, the tremendous expansion of Southern cotton growing was irrational. Wright and Howard C. Kunreuther, "Cotton, Corn, and Risk in the Nineteenth Century," *Journal of Economic History*, 35 (1975): 526-51. Higgs has rejected the argument because it is "not based on the von Neumann-Morgenstern axiom of rational behavior"; see Robert McGuire and Robert Higgs, "Cotton, Corn, and Risk in the Nineteenth Century: Another View," *Explorations in Economic History*, 14 (1977): 167-82; and Wright and Kunreuther, "Cotton, Corn, and Risk in the Nineteenth Century: A Reply," *ibid.*, 183-95.

<sup>31</sup> Harold Hoffsommer, *Landlord-Tenant Relations and Relief in Alabama*, FERA Research Bulletin, 2d ser., no. 9 (Washington, 1935), 2. Also see Charles S. Mangum, *The Legal Status of the Tenant Farmer in the Southeast* (Chapel Hill, 1952). Particularly illuminating is his section entitled "Remedies provided for enforcement of landlords' liens"; *ibid.*, 435-43. As early as 1919 Max Weber recognized the existence of Southern debt peonage: "The Negroes are share tenants bound by debt. . . . Their freedom of movement exists only on paper." His discussion of the South indicates that he had read W. E. B. Du Bois. Weber, *General Economic History*, trans. F. H. Knight (1927; reprint ed., New York, 1961), 75-76, 272. Stephen J. DeCanio has criticized Ransom and Sutch's analysis of debt peonage; for the analysis, see Ransom and Sutch, *One Kind of Freedom: The Economic Consequences of Emancipation*, chap. 8; and, for the criticism, see DeCanio, "Cotton 'Overproduction' in Late Nineteenth-Century Southern Agriculture," *Journal of Economic History*, 33 (1973): 608-33. For Ransom and Sutch's reply, see "Economy Theory and Southern History," in Southern Economic History Project, *Working Papers Series*, no. 13 (Berkeley and Los Angeles, 1974), 8-13. Higgs has maintained that debt peonage could not have been widespread because it was irrational for planters; it would have promoted "a perpetual in-

Debt peonage was not limited to sharecroppers; nor were they necessarily more exploited and oppressed by the labor-repressive system than were cash renters, usually regarded as one step up the socioeconomic ladder. During the 1890s, when cotton prices reached their low point for the century, renting replaced sharecropping at an astonishing rate. Higgs, for one, has taken this shift to rental labor as a sign of progress, "a response to the growth in the number of experienced black farmers to whom landlords were willing to grant such contracts."<sup>32</sup> An alternative interpretation is more plausible: the economic collapse during that decade made it more profitable for landlords to collect rent instead of a share of the cotton crop from their tenants. Landlords, therefore, responded to the depression by forcing their tenants to assume the full extent of the risk, a risk in which the planters had previously shared. Landlords could still require tenants to obtain credit from them, thereby earning interest and tying their renters to the land by debt peonage until another season, when cotton might become profitable once again; then renters could be turned back into sharecroppers.<sup>33</sup>

The actions of the planter class during the Mississippi River flood of 1927 are revealing. High water covered fifty miles on each side of the channel, submerging the delta plantation district and driving four hundred thousand black tenants from their homes. The planters believed, according to William Alexander Percy, one of their leaders, that "the dispersal of our labor was a longer evil . . . than a flood."<sup>34</sup> They insisted that laborers not be permitted to leave the region so that the tenants could be returned to the plantations when the waters receded. The Red Cross and the National Guard operated refugee camps and helped the planters by acceding to their demand that the camps be "closed"—fenced and locked—so that the blacks could not get out and labor recruiters from other areas could not get in. The governor of Mississippi himself denounced labor recruiters who offered employment elsewhere to victims of the flood. Planters argued that, since labor contracts had already been signed for the 1927 season and since advances had been made to tenants, blacks had to go back and work after the flood, even though it became clear that the waters would not recede quickly enough to permit any planting. The Red Cross distributed emergency supplies not to the blacks inside the locked camps but to the local planters, some of whom billed their tenants after passing on the supplies, creating further indebtedness. The NAACP denounced the "peonage" practiced in refugee "concentration camps," but the planters succeeded in preventing blacks from leaving the region and in tightening the bonds that tied the tenants to their landlords.<sup>35</sup>

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debtedness" and would have meant "giving away real resources." Higgs, *Competition and Coercion: Blacks in the American Economy*, 58. This position, however, does not take into account the income from debt service nor does it weigh the added benefits of a guaranteed labor force.

<sup>32</sup> Higgs, *Competition and Coercion: Blacks in the American Economy*, 67.

<sup>33</sup> LaWanda F. Cox, "Tenancy in the United States, 1865–1900: A Consideration of the Validity of the Agricultural Ladder Hypothesis," *Agricultural History*, 18 (1944): 99–100.

<sup>34</sup> Percy, *Lanterns on the Levee* (New York, 1941), 258.

<sup>35</sup> Daniel, *The Shadow of Slavery*, 149–69.

The regional apparatus of “involuntary servitude” that prevailed between Reconstruction and World War II extended well beyond debt peonage; it also consisted of five different kinds of laws, all of which worked to restrict the free market in labor: enticement statutes, which made it a crime for one planter to hire laborers employed by another; emigrant agent laws, which severely restricted the activities of out-of-state labor recruiters; contract enforcement statutes, which made it a criminal offense for tenants to break contracts with landlords; vagrancy statutes, drawn broadly enough to permit landlords to enlist the aid of local courts to keep laborers at work; and the criminal surety system, backed up by the system of convict labor, which permitted convicts to serve their sentences laboring for private employers. Enticement acts were revived in eight out of ten Southern states after Reconstruction and survived with amendments into the mid-twentieth century. An Alabama statute from 1920 outlawed even attempted enticement. Like the enticement laws, emigrant agent acts were intended to control competition among white employers rather than to punish workers who moved. They sought to prevent the activities of out-of-state labor recruiters by levying prohibitive license fees. In the Carolinas, the license cost one thousand dollars per county, with a penalty for unlicensed recruiting of up to five thousand dollars or two years in prison. Six states of the Deep South passed such laws between 1877 and 1912, and three more did so between 1916 and 1929.<sup>36</sup>

Other laws limited the freedom of laborers to move. Vagrancy acts forced workers to sign labor contracts. Penalties and apparently enforcement as well increased between 1890 and 1910 in response to the rise of agrarian insurgency. Georgia’s law of 1895 provided for a fine of one thousand dollars or six to twelve months on the chain gang for those found without employment. The vagrancy acts permitted sheriffs to function as labor recruiters for planters, rounding up “vagrants” at times of labor shortage. Additional laws upheld labor contracts. Late in the nineteenth century six states passed “contract enforcement” and “false pretenses” statutes, which held that a worker’s unjustified failure to work constituted “*prima facie* evidence of the intent to injure or defraud the employer.” An Alabama law of 1903 did not permit the defendant to rebut testimony about his intentions.<sup>37</sup> Under “criminal surety” laws, employers were permitted to pay the fines of individuals convicted under contract enforcement or vagrancy proceedings; the convict had to work for that employer until his earnings repaid the fine. Thus a laborer whose work displeased his landlord could not only be convicted of a crime but also be compelled by the court to labor for the same employer. The alternative for convicts was the chain gang, and almost

<sup>36</sup> *General and Local Laws (and Joint Resolutions) of the Legislature of Alabama, passed at the Special Session of 1920* . . . (Montgomery, 1920), 155; *Laws and Resolutions of the State of North Carolina passed by the General Assembly at Its Session of 1891* . . . (Raleigh, 1891), 77; *Acts and Joint Resolutions of the General Assembly of the State of South Carolina, passed at the Regular Session of 1891* (Columbia, S.C., 1892), 1084; and William Cohen, “Negro Involuntary Servitude in the South, 1865–1940: A Preliminary Analysis,” *Journal of Southern History*, 42 (1976): 36, 39–40.

<sup>37</sup> *Acts and Resolutions of the General Assembly of the State of Georgia, 1895* (Atlanta, 1896), 63; *General Laws (and Joint Resolutions) of the Legislature of Alabama, passed at the Session of 1903* (Montgomery, 1903), 345; and Cohen, “Negro Involuntary Servitude in the South, 1865–1940,” 43, 48–49.



anything was preferable to its brutality. A distinctly Southern institution, it was reserved primarily for convicts who refused to sign criminal surety contracts or who were unable to get any landlord to pay their fine and hire them.

Thus, the Southern states established a net of laws to limit the mobility of labor. Vagrancy acts forced workers to sign labor contracts; contract enforcement and false pretenses laws prevented them from leaving. If they left nevertheless, the criminal surety system could return them to the employer, who was backed by the threat of the chain gang. Enticement and emigrant agent statutes prevented another employer from seeking their labor. In the North a laborer whose work displeased his employer could be fired; in the South he could be convicted of a criminal offense. This web of restrictive legislation distinguished the South's labor system from that of the classically capitalist North.<sup>38</sup>

How successful were these laws? It is difficult to tell. William Cohen has suggested that one measure is the extent of their litigation in higher courts, an expensive and time-consuming practice—undertaken, presumably, only if enforcement were of great importance. The Alabama criminal surety law came before the state supreme court at least sixteen times between 1883 and 1914, and the Georgia contract enforcement law was litigated in appellate courts on eighty different occasions between 1903 and 1921.<sup>39</sup> These cases suggest fairly extensive reliance on the law to repress labor, for those argued in the appellate courts were only the tip of the iceberg. The mere threat of prosecution usually sufficed to bring about the desired result; and, since only a handful of sharecroppers had the resources to appeal a conviction, planters, sheriffs, and local judges had virtually a free hand. Informal practice extended the law; extralegal and illegal acts were often undertaken to accomplish the same ends. In September 1901 local officials in the Mississippi black belt rounded up “idlers and vagrants” and drove them “into the cotton fields,” where the farmers were “crying for labor.” In February 1904 police in Newton, Georgia, made “wholesale arrests of idle Negroes . . . to scare them back to the farms from which they emanated.” In 1908 the steamer *America* docked at a Natchez wharf, seeking to recruit black laborers. White businessmen established a local committee, whose methods according to one Southern reporter were “so emphatic that the negroes concluded to abandon their idea of leaving.”<sup>40</sup>

Legal and illegal efforts to restrict the mobility of labor in the South did not, of course, completely succeed; they only made it difficult. But the planter class did not require that every laborer be tied to his landlord, only that most, too frightened to leave, remain in order to preserve the low-wage, labor-intensive system of production. The most resourceful, energetic, and determined were al-

<sup>38</sup> For parallel developments following emancipation in Latin America, see C. Vann Woodward, “Emancipations and Reconstructions: A Comparative Study,” paper delivered at the Thirteenth International Congress of Historical Sciences, Moscow, August 16–23, 1970; Carl Degler, *Neither Black nor White: Slavery and Race Relations in the United States and Brazil* (New York, 1971), 220–24; and Willemina Kloosterboer, *Involuntary Servitude since the Abolition of Slavery* (Leiden, 1960).

<sup>39</sup> Cohen, “Negro Involuntary Servitude in the South, 1865–1940,” 44–45, 54.

<sup>40</sup> *Atlanta Constitution*, September 17, 1901, and February 1, 1904; *Charleston News and Courier*, December 27, 1908; and Cohen, “Negro Involuntary Servitude in the South, 1865–1940,” 50, 40.



ways able to escape from their landlords and from the region, and more did so each year—but not because the planters made no effort to stop them. The typical departure occurred under cover of darkness, with family and neighbors sworn to secrecy. Large-scale black migrations from the South took place only twice between Reconstruction and the Depression: the “Kansas Fever” exodus of 1879–80 and the migration during World War I. Aside from these two movements, the migration rate from Southern states was significantly lower than that from other areas of the country, another measure of the success of repressive law and regional practice.

The dominant class responded to these two waves of outmigration by increasing physical terror and intensifying repressive legislation. A wave of “bulldozing” broke out in response to the Kansas exodus of 1879; according to Nell Painter, the planters’ favorite tactics were “imprisonment for debt and brute force.” When blacks from the Deep South headed for Kansas in massive numbers, Southern whites first assassinated the leaders, then beat and lynched their followers.<sup>41</sup> One witness told a Congressional committee that in Alabama “the bulldozers killed off all the colored men they knew intended going to Kansas”—over a hundred died in one county alone. The second great black exodus from the South, during World War I, led planters to intensify their efforts. Three states passed new emigrant agent laws, and three others drastically increased the severity of existing statutes. Alabama’s 1919 law required labor recruiters to pay an annual license fee of five thousand dollars per county and supply statements of their good moral character signed by twenty “householders and freeholders”; each applicant had to have been a state resident for six months. These provisions applied not only to labor agents but to assistants, messengers, and “even the printer who ran off recruiting handbills.”<sup>42</sup> Such laws undoubtedly discouraged programs like those of the Pennsylvania and Erie railroads to recruit and transport trainloads of Southern blacks for labor in the North. As usual, informal practice supplemented law: in Savannah police arrested every black found in the railroad station; in Greenville police dragged blacks from trains; in Hattiesburg a railroad employee refused to sell tickets to blacks.<sup>43</sup>

The Populist movement was a response to the special form of oppression under which Southern farmers labored. The Farmers’ Alliance recognized debt peonage as a key source of exploitation; the organization sought to fight it with cooperative buying and selling schemes and with the subtreasury plan, which

<sup>41</sup> Painter, *Exodusters: Black Migration to Kansas after Reconstruction* (New York, 1976), 196, 192; and Herbert Gutman, *The Black Family in Slavery and Freedom, 1750–1925* (New York, 1976), 435, 438–39.

<sup>42</sup> “Report of Testimony of the Select Committee . . . to Investigate the Causes of the Removal of the Negroes from the Southern States to the Northern States,” Senate Report no. 693, 46th Congress, 2d Sess., 1 (Washington, 1880): 413; *General Laws (and Joint Resolutions) of the Legislature of Alabama, passed at the Session of 1919* . . . (Montgomery, 1919), 187–88; and Cohen, “Negro Involuntary Servitude in the South, 1865–1940,” 39–40.

<sup>43</sup> George B. Tindall, *The Emergence of the New South, 1913–1945* (Baton Rouge, 1967), 147, 149. Restricting black migration to the North was compatible with the views of Northern workers, who feared from the beginning of the Civil War that “hordes of Negro laborers . . . unleash[ed] from the plantations . . . would depress industrial wage levels” in the North. Midwestern farmers similarly feared black competition for Northern farm land. See David Montgomery, *Beyond Equality: Labor and the Radical Republicans, 1862–1872* (New York, 1967), 91; and Leon Litwack, *North of Slavery: The Negro in the Free States, 1790–1860* (Chicago, 1961), 153–86.

promised government money for cooperative exchanges to use as an advance against crops. Populist involvement with the currency question was at bottom a recognition of debt peonage as a bond of oppression that tied Southern farmers, black and white, to the region's repressive labor system. To abolish the lien system of financing cotton production, growers would have had to abolish "the machinery of exploitation," as C. Vann Woodward has put it. The Populist threat from below helped forge the alliance of industrialists and planters in the South; this reactionary coalition intensified the repressive control of labor and struck devastating blows at democracy by disfranchising thousands of blacks and poor whites as well.<sup>44</sup>

While an extensive network of formal and informal obstacles to the free market in labor thus distinguished the South's labor-repressive economy from the North's classic capitalism, additional differences separated the two systems. Unlike the true agricultural proletarian, the Southern share tenant working on "thirds and fourths" claimed his portion of the product not only on the basis of his labor but also as possessor of part of the instruments of production: the Southern share tenant provided his own tools and draft animals; the Northern agricultural wage worker, "freed" of possession of the means of production, provided only his labor. In this respect the Southern tenant was closer to the traditional peasant than to the Northern farm laborer. Woodman has argued that the postwar South was moving away from this kind of tenancy, toward share-wages, in which the laborer was indeed stripped of everything except his labor; the planter provided the land as well as the tools and animals, and the laborer was paid a wage in the form of a share of the crop.<sup>45</sup> Even if there was a trend toward share-wages (and its extent is not clear), the absence of classic capitalist class relations counterbalanced it. The system that limited the region's free market in labor in fact prevented Southern sharecroppers from becoming free wage laborers.

The contrast between the share-wages of the 1880s and those of the immediate postwar years is illuminating. In 1865 and 1866 those who provided only their labor drew wages in the form of a share of the crop, but they labored under overseers in gangs on a plantation that was organized as a single, centralized productive unit. The division of this centralized unit into small tenant farms, the substitution of family for gang labor, the end of constant supervision by overseers and the substitution of intermittent visits by the landlord himself, the loss of economies of scale and the end of centralized management—all of these developments marked not the creation of "large-scale, thoroughly capitalistic farms" but precisely a move away from the classic capitalist organization of agriculture.

<sup>44</sup> Lawrence Goodwyn, *Democratic Promise: The Populist Moment in America* (New York, 1976), 117–53; David Montgomery, "On Goodwyn's Populists," *Marxist Perspectives*, 1 (1978): 166–73; C. Vann Woodward, *Tom Watson, Agrarian Rebel* (New York, 1963), 132; Sheldon Hackney, *Populism to Progressivism in Alabama* (Princeton, 1969), 147–229; and J. Morgan Kousser, *The Shaping of Southern Politics: Suffrage Restriction and the Establishment of the One-Party South, 1880–1910* (New Haven, 1974), chaps. 4–5.

<sup>45</sup> Woodman, "Sequel to Slavery," 552.

The resulting pattern of development was distinct from that of the North in that it was not based on a growing mechanization of agriculture, an increasing division of labor, and a substitution of capital-intensive for labor-intensive operations. In the North, systematic increases in labor's productivity enabled the dominant class to increase the surplus. This "qualitative" transformation of production, as Genovese and others have argued, was based on the investment of surplus in technological advances that made labor more productive.<sup>46</sup> The family farm, owned by a small entrepreneur who steadily increased his efficiency by investing in technological improvements, was the economic basis of a rural region growing in prosperity and population; this growth in turn provided a strong market for manufactured goods and created an expanding commercial network. The resulting mutual development of productivity in agriculture and industry is the classic capitalist path that had been blazed by England in the seventeenth and eighteenth centuries and followed by the Northern states. It is not the road the South took.

The dominant class in the immediate postwar South had two possible responses to a shortage of labor, at least in the abstract. It could follow England and the North, develop capital-intensive methods of production, and increase the productivity of labor. The alternative was to rely on the coercion of labor, to extract a larger surplus not by increasing productivity but by squeezing more out of the laborers. Because this intensification was likely to provoke resistance and flight, the second route required restrictions on labor mobility—formal laws and informal practices that tied the workers to the land and limited their access to alternative employment. This second route is not necessarily economically stagnant; it can bring economic development (as South Africa has shown with a vengeance), but in a manner distinct from the classical capitalist method. Eugene Genovese and Barrington Moore, Jr., mindful of European developments, have each called it the "Prussian Road" to modern society: development that preserves and intensifies authoritarian, repressive social relations.<sup>47</sup>

The possibility that the South could take the classic capitalist road was not ignored in the immediate postwar period. Some of the most astute Southerners pushed for just such a solution to the problems of postwar agricultural adjustment. The Selma *Southern Argus*, for one, argued tirelessly in the late 1860s that

<sup>46</sup> Eugene D. Genovese, *The Political Economy of Slavery* (New York, 1965), 48–51.

<sup>47</sup> Genovese, *The Political Economy of Slavery*, 206–07, *The World the Slaveholders Made* (New York, 1969), 226–30, and *In Red and Black* (New York, 1971), 346–47; and Moore, *Social Origins of Dictatorship and Democracy: Lord and Peasant in the Making of the Modern World* (Boston, 1966), chap. 8. For a discussion of the differences in interpretation between Genovese and Moore, see my "Review of Reviews: *Social Origins of Dictatorship and Democracy*," *History and Theory*, 15 (1976): 146–75. Analyzing the postwar South in terms of the Prussian road gives new meaning to C. Vann Woodward's argument that the region has much in common with the twentieth-century developing world; see Woodward, "The Irony of Southern History," in his *The Burden of Southern History* (Baton Rouge, 1960), 167–91. On South African parallels, see George M. Fredrickson, "South Africa and the American South," *Inquiry*, November 21, 1977, pp. 14–16. For a study of European development to which I am greatly indebted, see Robert Brenner, "Agrarian Class Structure and Economic Development in Pre-Industrial Europe," *Past & Present*, no. 70 (1976): 30–74; and Temma Kaplan, *Anarchists of Andalusia, 1868–1903* (Princeton, 1977). Also see Douglas F. Dowd, "A Comparative Analysis of the Economic Development in the American West and South," *Journal of Economic History*, supplement to 16 (1956): 558–74.

the planter class should end its reliance on labor-intensive methods of producing cotton and, instead, diversify crops, introduce stock-raising, and substitute labor-saving machinery for tenant labor.<sup>48</sup> The planter class took the other route, the Prussian road. Examining the social origins of the postwar planter class is necessary to understanding this choice. According to the theory of an "evolving bourgeois society," the old planters were "gone with the wind" and the postwar landlords were bourgeois merchants, who should have been open to classical capitalist development. Historians positing this interpretation would tend to agree with the reporter for the *Mobile Register* who claimed in 1871 that "not one of the colossal planters survives. . . . The old aristocracy, proud and exclusive, yet generous and chivalric, have disappeared. Their places are desolate, and in their ornamented grounds the owl and fox have their nests." But others had different perceptions. Henry W. Grady, for example, noted in 1881 a "sure though gradual . . . tendency toward the reestablishment of a landholding oligarchy. Here and there through all the cotton states . . . are reappearing the planter princes of the old time, still lords of acres though not of slaves."<sup>49</sup>

Grady was closer to the truth. A substantial proportion of the prewar planter elite survived the war with their landholdings intact or even expanded. Thus, the old families persisted in a quantitative sense.<sup>50</sup> In a qualitative sense, however, their relationship to production had been transformed. The postwar planters constituted a new class because they were in new social relations of production. But these relations were still distinct from those that prevailed in the North. The social roots of the postwar planters help explain why the South took the Prussian rather than the classical capitalist road after the Civil War. And, once the institutions of a labor-repressive system had been established, the planters had little incentive to mechanize or use other techniques to increase efficiency and productivity, as Jay R. Mandle has skillfully shown.<sup>51</sup> While wheat-growing farmers in the North responded to a shortage of labor with a technological revolution, planters in the Deep South in 1900 and even in 1930 relied on the coercive Prussian road; in consequence, Southern crop outputs, yields per acre, and agricultural technology changed little from year to year.<sup>52</sup>

<sup>48</sup> See, for example, Selma *Southern Argus*, August 11, 1869, and January and February, 1875, *passim*. Also see Paul M. Gaston, *The New South Creed: A Study in Southern Mythmaking* (New York, 1970), 66–88.

<sup>49</sup> *Mobile Daily Register*, January 25, 1871; and Grady, "Cotton and Its Kingdom," *Harper's New Monthly Magazine*, 63 (1881): 719–34.

<sup>50</sup> Of the 236 biggest landholders in the Alabama black belt in 1860, 43 percent remained in the area's landholding elite in 1870; the corresponding figure for 1850 to 1860 was 47 percent. The share of wealth in real estate held by the top 10 percent of black-belt planters increased between 1860 and 1870 from 55 to 63 percent; see my *Social Origins of the New South: Alabama, 1860–1885* (Baton Rouge, 1978), 9–16.

<sup>51</sup> Mandle, *The Roots of Black Poverty: The Southern Plantation Economy after the Civil War* (Durham, N.C., 1978), 60–70.

<sup>52</sup> This analysis also contributes to the debate over the continuity of Southern distinctiveness. The existence of fundamental socioeconomic differences between South and North was not limited to the antebellum period; the postwar South had a different type of capitalist development. But the South's distinctiveness is not "continuous and without serious interruption," as Carl Degler has recently argued; the postwar South's social relations not only were different from slave relations but were themselves transformed into a new type after the 1930s. See Degler, *Place over Time: The Continuity of Southern Distinctiveness* (Baton Rouge, 1977), xii.

THE MOST FREQUENT OBJECTION TO THIS ANALYSIS has been that technical obstacles to mechanizing cotton growing dictated the labor-intensive nature of Southern agriculture. In this view, the difference between Northern and Southern patterns of economic development arose not out of different class structures but out of natural differences between the regions. Geography dictated different crops with different possibilities for mechanization, which in turn led to different rates of development. In other words, the limits to Southern growth were physical rather than social: Southern soil and climate made mechanization along Northern lines impossible until the twentieth century, when scientific advances at last overcame natural obstacles.<sup>53</sup>

The mechanization of cotton growing was not a single process; cotton agriculture required three distinct operations—plowing, weeding and thinning, and picking—each of which had separate possibilities for mechanization. Under the labor-intensive sharecropping system, unskilled workers performed all three using animal power and simple tools: mule-drawn, single-row plows, hoes for weeding and thinning, and picking by hand. Examination of these three operations suggests flaws in the conventional explanation.

The replacement of the mule-drawn plow with the tractor provides the clearest evidence that obstacles to the mechanization of cotton were social rather than technical. The gasoline tractor, the symbol of farm mechanization, was developed in the 1890s and went into factory production in 1903. The farm labor shortage in the North during World War I spurred mass production and widespread use of the new machine in grain farming. Although the tractor was eminently suited to plowing the cotton fields of the Deep South, its introduction was delayed until World War II. In 1920, the North Central states had almost six times as many tractors per acre of cropland as the cotton states of the Deep South,<sup>54</sup> because the South's labor-repressive system provided no incentive to mechanize. Quite the reverse. For planters, mechanization had to be an all-or-nothing procedure. To use labor-saving machines for part of the season was pointless when planters were determined to keep their labor force available throughout the year. In Texas and Oklahoma, where seasonal wage labor rather than resident tenants cultivated cotton, tractors were introduced at a rate similar to that of the Midwestern wheat region. These two states had three times as many tractors per acre of cropland in 1940 as the Deep South states, with 44 percent of all tractors in the ten Southern states in Texas alone.<sup>55</sup>

The technology for mechanical plowing was simple compared to the requirements for mechanical harvesting, and historians generally have argued that har-

<sup>53</sup> This argument, made most emphatically by Julius Rubin, has been endorsed by Harold Woodman and Gavin Wright. See Rubin, "The Limits of Agricultural Progress in the Nineteenth Century South," *Agricultural History*, 49 (1975): 362–73; Woodman, "New Perspectives on Southern Economic Development," *ibid.*, 378–80; and Wright, *Political Economy of the Cotton South*, 180. Other explanations emphasizing natural factors focus on, for example, the boll weevil; see Ransom and Sutch, *One Kind of Freedom: The Economic Consequences of Emancipation*, 172.

<sup>54</sup> James H. Street, *The New Revolution in the Cotton Economy: Mechanization and Its Consequences* (Chapel Hill, 1957), 95, 162.

<sup>55</sup> *Ibid.*, 162, 159.



vesting was the real obstacle to mechanization of cotton agriculture. Yet proponents of the technological argument have overlooked the social prerequisites for technological innovation. Planters lacked incentives to mechanize; they organized their plantations on the basis of a guaranteed supply of low-cost labor tied to the land. Mechanization threatened to disrupt, and eventually to destroy, a sociopolitical order that they had created and perpetuated only with great effort. When challenged, especially by the Populists, planters had fought to preserve the institutions of the Prussian road; why should this generation suddenly dismantle them?<sup>56</sup> The agricultural implement industry understood the situation; at the same time that it worked tirelessly on wheat combines for the classically capitalist farmers of the Midwest, it devoted little effort to developing a cotton harvester for planters in the South.

Proponents of the technological argument have also overlooked the important research of Heywood Fleisig, who has argued that the technology for accelerated cotton harvesting was not only available but potentially profitable in the late nineteenth century. Isolated inventors who lacked the resources fully to develop their ideas patented no less than 242 cotton harvesters in the nineteenth century; one patented the basic principle of the modern picker in the 1890s.<sup>57</sup> Proponents of the technological argument have assumed that planters abolished labor-intensive methods as soon as technological advances in cotton harvesting made them unnecessary. But radical differences between regions in the use of mechanical pickers suggest that such was not the case. In 1955, 67 percent of the cotton crop was mechanically harvested in California, 24 percent in Texas, 2 percent in Alabama and Georgia.<sup>58</sup>

The introduction of machinery did not cause the breakup of the labor-intensive system of production; on the contrary, the collapse of the prevailing structure eventually led the planters to mechanize. This collapse was, of course, part of the world depression of the 1930s.<sup>59</sup> At the same time that the price of raw cotton fell to its lowest levels since before the Civil War, the black population of the cotton South was increasing. The birthrate in the rural Southeast was the highest in the nation in the two decades following World War I. Despite some migration out of the South during and after that war, the Southern black population rose steadily from 7.9 million in 1900 to 9.9 million in 1940.<sup>60</sup> The collapse of cotton demand, combined with the growing labor pool, presented a sufficient reason for change. By abandoning the labor-repressive system that tied blacks to the land, the planters could free themselves of responsibility for the year-round subsistence of their sharecroppers. Blacks could be hired on wages when needed and left to fend for themselves the rest of the year.

<sup>56</sup> To understand the planter class, "one must first understand the extreme reverence for the past with which each 'old family' is a link"; Allison Davis, Burleigh B. Gardner, and Mary B. Gardner, *Deep South: A Social Anthropological Study of Caste and Class* (Chicago, 1941), 84.

<sup>57</sup> Fleisig, "Mechanizing the Cotton Harvest in the Nineteenth-Century South," *Journal of Economic History*, 25 (1965): 704–06; and Street, *The New Revolution in the Cotton Economy*, 117–18.

<sup>58</sup> Street, *The New Revolution in the Cotton Economy*, 167.

<sup>59</sup> *Ibid.*, 42–48.

<sup>60</sup> Rupert B. Vance, *All These People: The Nation's Human Resources in the South* (Chapel Hill, 1945), 66–76, 233.



But the planters did better than that. The federal government paid them for not planting cotton and took over some responsibility for the subsistence of their labor force through relief and work programs. New Deal farm policy combined acreage reduction with parity payments to farmers. The Agricultural Adjustment Act (AAA) of 1933, substantially re-enacted in 1938, took 53 percent of the South's cotton acreage out of production and paid subsidies to the landowners.<sup>61</sup> Tying tenants to those 7.7 million fallow acres no longer made sense, and planters released them from their bonds. These sharecroppers joined the general pool of unemployed wage workers, seeking federal relief funds and further increasing the labor surplus.

Under the AAA acreage reduction program, the sharecropper who was to receive half the crop was entitled to half of the government payments for plowing it under. Wage laborers, however, had no claim on payments. Consequently, many planters refused to sign sharecropping contracts with tenants in 1934 but hired them as wage laborers instead.<sup>62</sup> These developments help explain why the number of Southern sharecroppers fell by 20 percent between 1935 and 1940, while the number of wage workers in Southern agriculture increased by almost 50 percent. With abundant labor at rock-bottom prices, the planters had no interest in mechanization, and by 1940 the South still had 3.7 million mules and horses but only 223 thousand tractors.<sup>63</sup> Thus, by the eve of the Second World War, the collapse of cotton markets and the growth of the labor force, combined with government agricultural policy, had dealt severe blows to the system of labor-repressive production; and wage labor, which at this point meant unemployment for hundreds of thousands, had to a significant extent replaced sharecropping and peonage.

Relaxing the bonds of labor-repressive agriculture had an immediate consequence: the migration of hundreds of thousands of blacks out of the South. The tide of black migration to the North did not begin with World War II and the opportunities it offered; that move was already underway during the De-

<sup>61</sup> *Ibid.*, 85; and Tindall, *Emergence of the New South, 1913-1945*, 392-94.

<sup>62</sup> D. E. Conrad, *The Forgotten Farmers: Sharecroppers in the New Deal* (Urbana, 1965), 50-63, 76-80; and Arthur F. Raper, *Preface to Peasantry* (Chapel Hill, 1936), 249-53. The typical cotton planter in 1935 received 15 percent of his net income from the AAA; T. J. Woofiter, Jr., et al., *Landlord and Tenant on the Cotton Plantation* (Washington, 1936), xxxi. Also see Raymond Wolters, *Negroes and the Great Depression: The Problem of Economic Recovery* (Westport, Conn., 1970), 27-28; Howard Hoffsommer, "The AAA and the Sharecropper," *Social Forces*, 13 (1935): 494-95; Arthur M. Schlesinger, jr., *The Politics of Upheaval* (Boston, 1960), 431; and Frank Freidel, *F.D.R. and the South* (Baton Rouge, 1965), 63-64. Other planters did not bother with the formality of turning their tenants into wage laborers; they simply forced them to hand over government payments. When one tenant refused, his landlord "called him up before the other tenants, showed him a clasp knife, and told him that he 'was going to cut his throat from ear to ear' unless he signed the check immediately. The tenant endorsed the check in favor of the landlord." This episode was reported, incidentally, by the landlord. Davis, Gardner, and Gardner, *Deep South: A Social Anthropological Study*, 398. Within the AAA the principal attorney of the legal division fought a losing battle to protect tenants; his name was Alger Hiss. Conrad, *Forgotten Farmers: Sharecroppers in the New Deal*, 56-58.

<sup>63</sup> Vance, *All These People: The Nation's Human Resources in the South*, 216-17, 200. The census of agriculture from 1935 shows tenancy increasing in all regions of the country except the South, where there was a significant decline in tenancy and an increase in wage labor; W. A. Turner, *A Graphic Summary of Farm Tenure*, U. S. Department of Agriculture Miscellaneous Publication no. 261 (Washington, 1936), 25. In Georgia the number of tractors per thousand acres of cropland harvested rose from 0.7 in 1930 to 1.1 in 1940, compared to 9.4 in the Middle Atlantic states in the latter year; by 1950 the figure for Georgia reached 8.4; Street, *The New Revolution in the Cotton Economy*, 159, 162.

pression, as the Prussian road crumbled. The net migration of whites from the Southeast between 1930 and 1940 was less than one thousand; the net migration of blacks was four hundred and twenty-five thousand<sup>64</sup>—strong evidence against the argument that lack of opportunities elsewhere was the principal obstacle to black mobility. Clearly, blacks were moving to the North during the Depression, when jobs there were scarce. Thus, developments in the North were neither necessary nor sufficient causes for black migration; to understand it, one must begin with the place of blacks in the social relations of the South.

Despite the loosening of bonds on black labor during the Depression, planters fell back on their old coercive ways when they thought the occasion warranted. The bumper cotton crop in 1937 was such an occasion. Having forced their tenants onto public relief, the planters' old fear of a labor shortage returned. A mob entered Warrenton, Georgia, and forced Negroes into the fields, while elsewhere in Georgia some planters, when they found neighbors competing for their pickers, "carried guns and fired them into the air. They told the pickers there was plenty of cotton to pick in [the] . . . county and asked them to stay home and pick it." The blacks stayed.<sup>65</sup> In Anderson, South Carolina, the police used the vagrancy laws once again to round up laborers for the harvest, and planters successfully pressured the WPA to release its workers at harvest time so that a labor shortage would not aid the Southern Tenant Farmers Union in its effort to organize.<sup>66</sup>

A surplus of labor existed after 1937, despite the great migration, but it abruptly disappeared during World War II. The South's farm population decreased by 20 percent between 1940 and 1945; Mississippi alone lost twenty-eight thousand sharecroppers.<sup>67</sup> This movement away from the cotton fields was possible only because the combination of depression labor surplus and planter-supported government policy had sharply eroded the restrictions on labor mobility. Wages in cotton increased rapidly during the war, firm evidence that restrictions on labor mobility that had guaranteed an abundant supply of cheap labor no longer existed. The wage for picking one hundred pounds of cotton rose from \$.62 in 1940 to \$1.93 in 1945, an increase of 211 percent. The planters, stunned by the new bargaining power of their former peons under the laws of the free market, turned to the federal government for help. The wage stabilization board obligingly ended the freedom of workers to demand high prices for their labor, setting a wage ceiling for cotton harvesters in 1945 and 1946 in Mississippi, Texas, and Arkansas. When wartime wage controls ended, cotton har-

<sup>64</sup> Vance, *All These People: The Nation's Human Resources in the South*, 126. The census figures on interstate migration are not particularly relevant here, because they do not distinguish movement between states in the South from movement out of the South altogether.

<sup>65</sup> Tindall, *Emergence of the New South, 1913-1945*, 480; *New York Times*, September 16, 1937, p. 1; and Cohen, "Negro Involuntary Servitude in the South, 1865-1940," 59.

<sup>66</sup> W. J. Cash, *The Mind of the South* (New York, 1941), 419-20; and Tindall, *Emergence of the New South, 1913-1945*, 479-80. On the STFU, see Donald H. Grubbs, *Cry from the Cotton: The Southern Tenant Farmers Union and the New Deal* (Chapel Hill, 1971).

<sup>67</sup> Street, *The New Revolution in the Cotton Economy*, 213. Of course, not all blacks who ceased to be sharecroppers left the rural South; many became wage laborers in the cotton fields. Those who remained sharecroppers received more favorable tenure arrangements under the new conditions of labor shortage.

vesters' wages rose further to \$2.90, 360 percent more than in 1940, while in Mississippi the 1948 wage was 470 percent above that of 1940.<sup>68</sup>

Only at this point, after restrictions on labor mobility had disappeared and the labor shortage had driven wages to undreamed-of levels, did planters in the Deep South finally turn to mechanization. International Harvester, perceiving the changes that were underway, announced in 1942 the beginning of commercial production of a cotton harvester; two years later Allis-Chalmers obtained the patent rights to the most successful picker. Tractors now entered the South at an amazing rate. Their numbers in Georgia increased from nine thousand in 1940 to almost twenty-five thousand in 1945 to sixty thousand in 1950. Planters who introduced tractors took a decisive step away from re-establishing labor-repressive methods of production. Once the tractors did the plowing, the planter had little reason to coerce tenants into remaining on the land all year; instead, he now hired different wage laborers in different seasons. By 1950 the tractor was a characteristic feature of the Southern cotton farm, and 1955 marks the beginning of the final phase, mechanization of the cotton harvest.<sup>69</sup>

The "evolving bourgeois society" approach sees the South's rapid development during the 1950s and 1960s as the acceleration and culmination of a process of modernization that had begun at the end of the Civil War. In this view, the postwar South evolved more slowly than did the North, suffered temporary setbacks in the depressions of the 1890s and 1930s, but after World War II finally reached the take-off toward which it had been moving steadily since 1865. In fact, the origins of the contemporary South lie much closer to the present—in the 1930s. The Depression of that decade was not a temporary setback that delayed the South's socioeconomic evolution; it was a crisis that brought about the collapse of the labor-repressive system and its transformation from the Prussian into the classic capitalist road to development. The changes that accompanied World War II assured that the transformation was irreversible. Southern development from the Civil War to the present is thus not the evolution of a single system, from birth after 1865 to maturity a century later; it consists, instead, of two distinct types—two different social formations in two different periods—separated by the crisis of the Great Depression. The transformation of Southern social and economic life that occurred between 1930 and 1945 is thus second in significance only to that produced by the Civil War.

TOO MUCH OF THE RECENT DEBATE has focused on the question of Southern socioeconomic development, treating politics as a separate question. The Prussian road links the two. Both the South's characteristic poverty and its political op-

<sup>68</sup> Street, *The New Revolution in the Cotton Economy*, 204, 201.

<sup>69</sup> *Ibid.*, 130–31, 159; and Richard H. Day, "The Economies of Technological Change and the Demise of the Sharecropper," *American Economic Review*, 57 (1967): 429–30, 438. Numerous transitional forms of tenure existed for brief periods; for a description of these forms, see Street, *The New Revolution in the Cotton Economy*, 211–32. For a similar argument emphasizing labor factors rather than technological considerations in the wheat belt, see Paul A. David, "The Mechanization of Reaping in the Antebellum Midwest," in Robert W. Fogel and Stanley L. Engerman, eds., *The Reinterpretation of American Economic History* (New York, 1971), 214–27.

pression arose out of the same social relations. They posed a major obstacle not only to the development of a classically capitalist growth economy but also to liberal democracy as well. The South's departure from the Prussian road, for the most part completed by the early 1950s, was followed by intense and largely successful efforts to end black political disfranchisement and racial segregation and to achieve civil rights for blacks. The relaxation of direct coercion by landlords and the state in the productive process gave blacks more freedom to fight for political democracy. That their struggle required a massive social movement is evidence of the strength of the coercive apparatus even after its direct economic role had ended. Just as the Prussian road united economic with political repression, so the newly won economic freedom of the classic capitalist path created the opportunity for blacks to organize and win democratic reforms.<sup>70</sup>

Woodman has demonstrated the value of interpreting the postwar South in terms of "the emergence of new classes and new class relationships." He has correctly emphasized the importance for socioeconomic history of "the special southern form of wage labor." His analysis of its precise nature, however, has tended to obscure the distinctiveness of the South's class relations. Only in a limited sense has the postwar South gone "through the process of social change, of modernization, that the rest of the nation had gone through half a century or more earlier."<sup>71</sup> The rest of the nation did not become a capital-intensive, increasingly productive society by passing through a stage of large landholdings worked by sharecroppers under a coercive labor system. The South between 1865 and 1900 was neither an immature form nor an incomplete version of the North; during this period it was not evolving into or developing toward the North. Although the South had fewer factories, smaller cities, and less wealth than did the North, its socioeconomic relations constituted a fully formed system that was qualitatively different from the North's classic capitalism. The distinctiveness arose out of the labor-repressive nature of Southern production and the direct participation of the state in enforcing restrictions on the mobility of labor. The social relations of the Prussian road not only guaranteed an abundant supply of low-cost labor for the dominant class, eliminating the incentives to mechanize agriculture, but they were also incompatible with liberal democracy. The system was strong enough to prevent the rise of Northern-styled capitalist relations for sixty-five years after the destruction of slavery. Only after the shocks of the Great Depression of the 1930s and World War II could the South move off the Prussian road and join the North in classic capitalist development, with its distinctive freedoms and its distinctive forms of oppression.

<sup>70</sup> For a particularly illuminating discussion of the social prerequisites for liberal reform, see Fredrickson, "South Africa and the American South," 16.

<sup>71</sup> Woodman, "Sequel to Slavery," 552-54.

## *Comments:*

JONATHAN M. WIENER WANTS TO SHOW that “class analysis . . . provides a more satisfactory basis for understanding the postwar South than neoclassical economics.” In this endeavor he is anything but neutral. Writing not as a disinterested scholar but as an advocate, he tries to “make a case” for his point of view. Like an unscrupulous trial lawyer, he does not shrink from misrepresenting, distorting, or suppressing facts if doing so advances his argument; nor does he hesitate to impugn the motives and character of those with whom he disagrees. Whatever its merit in the courtroom, this style of advocacy is an improper and counterproductive mode of expression in historical writing.

HISTORIOGRAPHICAL ESSAYISTS IN PARTICULAR have an obligation to render accurately the substance of the works they discuss. Clearly, such debating devices as gross misrepresentation, caricature, concealed omission, quotation out of context, and unwarranted attribution of motive are improper. In his exegesis of some recent books and articles by Joseph D. Reid, Jr., Stephen J. DeCanio, and Robert Higgs—research he finds utterly without virtue—Professor Wiener fails to meet this simple standard of honesty. Consider some examples. When he refers to Reid’s view of sharecropping as entailing “‘optimum distribution of management talent among tenants’—that is, white management of black tenants,” Wiener simultaneously misreads the content of the quoted phrase, which compares only tenants and is silent about landlords, and gratuitously imposes on it a racial significance that Reid neither gave nor intended to give. Wiener’s statement that Reid’s article of 1973 lacks empirical evidence is false; in fact, a chief virtue of that article is its detailed discussion of the terms of actual farm rental contracts. Wiener’s description of DeCanio’s treatment of non-quantitative evidence could have made a valid point, but instead Wiener resorts to caricature. When Wiener reports that I attributed the growth of black income after 1865 to the blacks’ learning experiences “as sharecroppers under white landlords,” he is seizing upon and distorting a minor element in a complex, multivariate explanation and creating the impression that there is little to choose between me and U. B. Phillips.

Reid, DeCanio, and Higgs, we are told, view the postbellum economy as “a realm of equality, freedom, and achievement.” This unqualified description does an extreme disservice to DeCanio and Higgs, who have written at length of the South’s racial violence, coercion, and discrimination—in the economic as well as in the social and political spheres—and to Reid, whose work has been more narrowly confined to the study of land tenure arrangements and who has never even approximately stated the broad view of the postbellum economy ascribed to him. The same Terrible Trio is alleged to regard “decentralized family

sharecropping as the most rational organization of agriculture,” though the claim is not—and cannot be—documented. What Reid and I have said is that there are good economic reasons for certain parties to enter into sharing contracts under certain conditions, a position wholly different from the one attributed to us. Again, Professor Wiener claims that the Terrible Trio has not explained the shift from wages to sharing arrangements after the war, even though both Reid and I have written at length about this transition. Finally, I allegedly have rejected an argument of Gavin Wright and Howard C. Kunreuther “because it was ‘not based on the von Neumann–Morgenstern axioms of rational behavior.’” This statement not only omits several other, more important and more empirical, bases for my judgment but is also calculated to make me look silly in the eyes of those noneconomists who are unaware that the von Neumann–Morgenstern axioms are nothing more than requirements for internal consistency in a set of choices.

I could continue with such examples, but they are already tedious. The conclusion is clear. Through a succession of distortions and misrepresentations, Professor Wiener has suggested that the research of the Terrible Trio deserves unqualified contempt; he has insinuated that Reid, DeCanio, and Higgs are not only ideologists in economists’ clothing but racists as well. I personally resent this vicious innuendo. In any event, a holier-than-thou attitude makes no contribution to the pursuit of truth. Other reviewers of this research, who have their own reasons to dislike it, have nevertheless been able to remain polite and render its substance without gross distortion.<sup>1</sup>

EVEN MORE IMPORTANT THAN AN ACCURATE ACCOUNT of other scholars’ work is an accurate description of the historical facts at issue. Here, too, Professor Wiener’s essay repeatedly misses the mark. Consider the following assertions: (1) “the Populist movement was a response to the special form of oppression under which Southern farmers labored. . . . Populist involvement with the currency question was at bottom a recognition of debt peonage . . .”; (2) “the Southern share tenant provided his own tools and draft animals,” the planter provided the land; (3) “mutual development in productivity in agriculture and industry . . . is not the road the South took”; (4) “introduction [of the tractor] was delayed until World War II”; (5) “in Texas and Oklahoma . . . seasonal wage labor rather than resident tenants cultivated cotton”; and (6) “only at this point [the 1940s] . . . did planters in the Deep South finally turn to mechanization.” Every one of these assertions is either a misleading oversimplification or a gross misrepresentation of historical fact. In addition, Wiener would have us believe the the Ku Klux Klan was heavily occupied in suppressing the mobility of farm

<sup>1</sup> C. Vann Woodward, Review of Robert Higgs, *Competition and Coercion: Blacks in the American Economy, 1865–1914* (New York, 1977), in *Agricultural History*, 52 (1978): 194–95; and George B. Tindall, “The Costs of Segregation,” in Robert Haws, ed., *The Age of Segregation: Race Relations in the South, 1890–1945* (Jackson, Miss., 1978), 126–29.



laborers, that the Freedmen's Bureau supported planters more than freedmen, and that the coercive apparatus of the South remained substantially unaltered from the Civil War to the Great Depression. None of these intimations can withstand a confrontation with the full set of facts available to scholars. One must conclude that Wiener has little interest in the facts, since he has tried persistently to twist them to fit the interpretation he advocates.

Although Professor Wiener has raised some serious questions about the postbellum development of the South, he has muddied the waters with ideologically inspired pronouncements that must be cleared away in order to separate the specious from the real. Much of the discussion surrounding his appeal for a "class analysis" falls within the realm of the specious. When scholars are urged to base their analyses on "historically concrete social classes" rather than on "abstractly rational individuals," I do not know whether to laugh or cry. Such advice displays a notable lack of what my father—who farmed on shares in Oklahoma and Arkansas as a young man—always called "horse sense." In the same vein, one must disregard the tiresome and gratuitous insistence on the "oppression" of almost everyone, which culminates in the absurd paradox that the Southern farm laborer was forced to be unfree while the Northern farm laborer was "forced to be free"—but both were equally oppressed.

The notion of a Southern social structure with a small, tightly knit class of planters ruling over a huge, biracial proletariat, with class rather than race as the decisive division between the two, would not have amused blacks of the late nineteenth and early twentieth centuries. They knew who their enemies were, and the crackers stood at the top of the list. Class-conflict analysis, in the form urged here by Professor Wiener, is a hopelessly rigid and irrelevant framework for understanding the postbellum South. This is not to say that the region was free of significant conflict. Quite the contrary. But Wiener's proposed delineation of the classes, their motives and their behavior, and their modes of conflict simply does not fit the facts of Southern history. One cannot stuff just anything into a neo-Marxian trunk. Most significantly, important conflicts *within* the so-called ruling class will not fit. A central theme of my book, *Competition and Coercion: Blacks in the American Economy, 1865-1914* (1977), is that this kind of conflict played a crucial role in the postbellum South. Obviously, my message has not been received by everyone who claims to have read the book.

The issues in every instance are questions of more or less. They cannot be resolved by the all-or-nothing conclusions of the kind Professor Wiener would have us reach, but only by ascertaining, as J. H. Clapham long ago advised, how large, how long, how often, how representative. We need to know much more about many things: the credit system;<sup>2</sup> the system of land tenure;<sup>3</sup> crop se-

<sup>2</sup> Claudia Goldin, "'N' Kinds of Freedom: An Introduction to the Issues," *Explorations in Economic History*, 16 (1979): 8-30.

<sup>3</sup> Robert Higgs, "Race, Tenure, and Resource Allocation in Southern Agriculture, 1910," *Journal of Economic History*, 33 (1973): 149-69, and "Patterns of Farm Rental in the Georgia Cotton Belt, 1880-1900," *ibid.*, 34 (1974): 468-82; and Lee J. Alston, "Costs of Contracting and the Decline of Tenancy in the South, 1930-1960" (Ph.D. dissertation, University of Washington, 1978).

lection and methods of cultivation;<sup>4</sup> government, law, and quasi-legal coercion;<sup>5</sup> competition among employers, landlords, merchants, and creditors;<sup>6</sup> mechanization, fertilization, and other sources and means of gains in productivity;<sup>7</sup> and patterns of migration.<sup>8</sup>

As an approach to these complex issues, the class-conflict analysis that Professor Wiener proposes simply cannot accommodate the rich diversity of the actual historical experience, with its large variability among times, places, persons, races, and social groups within the postbellum South. A more promising approach is the mode of analysis demonstrated in the research of the scholars whose works I have cited above. This approach, which employs modern economic theory to derive refutable hypotheses and submits these hypotheses to statistical and other empirical tests, squarely confronts the facts in all of their diversity. The methodology is not foolproof; nor does it offer a means of escape from uncertainty and controversy—no methodology can do that. But it does offer a realistic hope of expanding our understanding of the postbellum South, as it has already demonstrated to those whose minds are not closed.

I cannot emphasize too much—the point has totally escaped Professor Wiener—that the crucial feature of this research is not its reliance on modern economic theory as a source of testable hypotheses, but its use of exacting empirical tests in evaluating them. How one tests a hypothesis is far more important than what led one to propose it in the first place, and the greatest contribution cliometric research has made to the study of Southern history has been the rigor it has brought to such testing. For decades historians have found it all too easy—as Wiener's essay makes clear—to “cook up” a plausible interpretation of Southern development. A fact here, a fact there, some scattered laws (never mind if they rarely obtained enforcement), a few easily accessible Southern horror stories about peonage, chain gangs, and the like: mix (up) well and blend with a dash of Marxian or Populist rhetoric; (half) bake under the hot Southern sun; and serve while still juicy. Such recipes can be concocted almost effortlessly. It is not as easy, however, to prepare an intellectually nourishing meal of reliable and systematic data, representative samples, carefully controlled statistical proce-

<sup>4</sup> Stephen J. DeCanio, *Agriculture in the Postbellum South: The Economics of Production and Supply* (Cambridge, Mass., 1975); Robert McGuire and Robert Higgs, “Cotton, Corn, and Risk in the Nineteenth Century: Another View,” *Explorations in Economic History*, 14 (1977): 167–82; Gavin Wright, *The Political Economy of the Cotton South: Households, Markets, and Wealth in the Nineteenth Century* (New York, 1978); and Robert Allen McGuire, “An Empirical Investigation of Farmers' Behavior under Uncertainty: Income, Price, and Yield Variability for Late Nineteenth-Century American Agriculture” (Ph.D. dissertation, University of Washington, 1978), chap. 4.

<sup>5</sup> Robert Higgs, “Race and Economy in the South, 1890–1950,” in Haws, *The Age of Segregation*, 89–116, and *Competition and Coercion: Blacks in the American Economy*.

<sup>6</sup> Robert Higgs, “Firm-Specific Evidence on Racial Wage Differentials and Workforce Segregation,” *American Economic Review*, 67 (1977): 236–45, “Racial Wage Differentials in Agriculture: Evidence from North Carolina in 1887,” *Agricultural History*, 52 (1978): 308–11, and *Competition and Coercion: Blacks in the American Economy*.

<sup>7</sup> Alston, “Costs of Contracting and the Decline of Tenancy in the South,” *passim*; Moses S. Musoke, “Technical Change in Cotton Production in the U.S., 1935–1960” (Ph.D. dissertation, University of Wisconsin, Madison, 1976); and Robert Higgs, “Tractors or Horses: Some Basic Economics in the Pacific Northwest and Elsewhere,” *Agricultural History*, 49 (1975): 281–83.

<sup>8</sup> William Edward Vickery, “The Economics of the Negro Migration, 1900–1960” (Ph.D. dissertation, University of Chicago, 1969); and Robert Higgs, “The Boll Weevil, the Cotton Economy, and Black Migration, 1910–1930,” *Agricultural History*, 50 (1976): 335–350, and “Race and Economy in the South, 1890–1950,” 98–99, 111–15.

dures, and tightly fitting regression equations with significant coefficients, all served on a solid, internally consistent theoretical platter. Such meals are the highest achievements of the cliometric chefs. If some choke on this fare, the problem lies not with the cuisine but with the diners.

FINALLY, LEST SOMEONE OVERLOOK THE OBVIOUS, I should note that Professor Wiener's essay is not really about Southern development. Though he aspires to "analyze [the postwar South's] economic and social development in a systematic and comprehensive way," his essay deals exclusively with the agricultural sector of the black-cotton-plantation belt of the Deep South. Nothing is said about the Upper South or about the nonagricultural sector of the economy or about the cities. Once again, a writer who wishes to portray the South in its poorest light has focused on the very place where the light was, indeed, the dimmest. This tactic is "stacking the deck" with a vengeance, as virtually any indicator of social or economic progress shows the plantation region at the lowest level. But there was a great deal more to the postbellum South than its cotton plantations; and the cotton plantations grew relatively less important over time. Cities grew, industries developed, railroads spread over the land, incomes advanced in step with the national rate of growth or faster, and, yes (despite what Wiener's ideology requires him to profess), not every ordinary working person under the Southern sun was oppressed. The South had its full share of misery; its cruelties, its violence, and its poverty were real enough. But it was not stagnant. And its people—men and women of good will, for the most part, who managed to improve their economic and social conditions in spite of tremendous obstacles—deserve, minimally, to have their stories told without deliberate distortion.

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JONATHAN M. WIENER'S ARTICLE is a welcome addition to the growing literature on the postwar South because he argues—quite rightly—that historians must not view the region's adjustment to defeat and black emancipation in narrow economic terms. He rejects the recent work of a number of cliometricians, insisting on a more holistic approach, a class analysis that goes beyond the "abstract logic of the market." Although I agree with this approach, I question the way in which Wiener has applied it to explain the political economy of the postwar South.

PROFESSOR WIENER'S CENTRAL INSIGHT comes from Barrington Moore, Jr., and, therefore, beginning any evaluation of his argument by examining Moore's thesis would seem appropriate. Yet this is not really necessary, because Wiener borrows only a very small part of Moore's thesis—and not the most important part at that. If I understand Moore correctly, he has described a "Prussian Road" (to capitalism, it should be noted, and not to a "qualitatively different" system) in which the traditional ruling class, the Junkers, turned economic affairs over to a growing bourgeoisie but kept political power in its own hands, an arrangement that the bourgeoisie, too weak to win political power and too fearful of organized working-class opposition, accepted. The result, according to Moore, was a repressive but economically growing capitalist society that lacked bourgeois liberties and the tradition that sustained them and, hence, easily slipped into fascism.

Whatever its merits as an explanation of German or Japanese history, this thesis—or, better, Professor Wiener's version of it—seems singularly inappropriate to explain the history of the American South after the Civil War. Actually, all that remains of Moore's thesis in Wiener's work is the idea of a repressive society. Wiener has dispensed with the coalition between Junker and industrialist, for as he argues here—and at greater length in his book, *Social Origins of the New South: Alabama, 1860–1885* (1978)—the Southern ruling class was made up of a persisting planter elite that resisted economic modernization. But, according to Moore, if there was a coalition, it was an alliance between the propertied interests of the North and South *after* the Civil War and one in which Southern interests were clearly subordinated to those of the North. Nothing like this appears in Wiener's work.

If all Professor Wiener is arguing is that the post-Civil War South was repressive, illiberal, and undemocratic, we may accept his characterization and be done with it.<sup>1</sup> But of course he insists on much more. He argues that the South had a "fully formed system that was qualitatively different from the North's classic capitalism," a distinctive system arising from "the labor-repressive nature of Southern production and the direct participation of the state in enforcing restrictions on the mobility of labor." This distinct system was "strong enough to prevent the rise of Northern-styled capitalist relations for sixty-five years after the destruction of slavery." According to Moore, the South of slavery "had a capitalist civilization, . . . but hardly a bourgeois one."<sup>2</sup> For Wiener, apparently, the postwar South had neither.

GIVEN HIS EMPHASIS ON SOCIAL CLASSES and his insistence that the South had a social system qualitatively different from that of the North, I am surprised that Professor Wiener fails to present a detailed discussion of the class structure of the

<sup>1</sup> Even this characterization obviously overstates matters because these adjectives (and the equivalent antonyms for the North) have usually been applied only to race relations at a time when the vast majority of blacks lived in the South.

<sup>2</sup> Moore, *Social Origins of Dictatorship and Democracy* (Boston, 1966), 121.

South or a clear indication of how the various Southern classes—landlords, industrialists, tenants, workers—differed from their Northern counterparts. I find Wiener's discussion of these questions inadequate and unconvincing—indeed, often incorrect. Space limitations allow me only to point to some of the problems and suggest alternative hypotheses.

The distinctive Southern system of political economy arose, according to Professor Wiener, from the lack of a free market in labor. Labor, instead of being free to move in response to market signals, was immobile, bound by legal and extralegal restrictions, the “most important” of which was “debt peonage.” This immobility of labor created a planter class that significantly differed from the Northern bourgeois and “turned the sharecropper into a kind of ‘bound’ laborer” that contrasted sharply with the free worker in the North.

The long discussion of debt peonage and various other forms of labor coercion clearly describes a vicious system of labor repression, but I question whether it describes a system of “bound” labor that differed enough from practices in the North to produce a different economic system. In the first place, Southern tenants and sharecroppers were not “bound.” They could—and, in fact, did—move, more often and more regularly than did their Northern counterparts.<sup>3</sup> Indeed, if the various local laws and other coercive methods had been fully enforced, the results would have been easily discernible: in areas of labor shortage (presumably the areas that would send recruiters and offer inducements to workers to move) wages or rental agreements would have been superior to those where there was a surplus. Not only is there no evidence of this difference, but the censuses reveal extensive mobility from east to west and, among blacks, movement from largely white to largely black areas.

Professor Wiener also considers “additional differences [that] separated the two systems,” North and South. He claims that a share tenant was not a “true agricultural proletarian” because he owned some of his tools of production, unlike the Northern worker, who “provided only his labor.” This assertion is incorrect in a variety of ways. First, no one would argue that a tenant was a proletarian, and Northern tenants, like those in the South, provided some or all of their farm equipment. Second, many Northern workers continued to own some of their tools of production; indeed, many still do. Carpenters and other building trades workers, mechanics, tool- and diemakers, and other skilled workers to this day own hand tools that are a requirement for the job. But, if the trend was (and continues to be) in the other direction, that is, for employers to provide the tools, it was as apparent in Southern as in Northern agriculture. The move toward sharecropping was exactly that. Sharecroppers owned no tools of production—all were provided by the landlord—and, like workers elsewhere in agriculture and industry, the sharecroppers did not own the product or even a share of it. All they could claim was a right to their pay, which was usually a right to their share of the proceeds of the crop rather than a portion of the crop itself. In

<sup>3</sup> E. A. Goldenweisser and Leon E. Truesdell, *Farm Tenancy in the United States*, U. S. Department of Commerce, Bureau of the Census, Census Monograph no. 4 (Washington, 1924), 137.

short, the sharecropper was a wage worker, as every Southern state clearly recognized in practice and in law.<sup>4</sup>

The manner in which sharecropped plantations were run clearly demonstrates that the sharecropper was a wage worker. Professor Wiener argues, correctly, that cotton production remained labor-intensive but errs when he contends that on sharecropped plantations supervision was limited to "intermittent visits by the landlord" and that business affairs were marked by the lack of "centralized management." On large, sharecropped plantations like those in the Mississippi Delta, supervision was regular and close (including bells to mark work and meal times); foremen or "riders," as they were called, were constantly in the fields directing work. Plantation management was in the hands of professionals who assessed the work, purchased everything needed for production (tools, seed, fertilizer, and boll weevil poison, for example), directed their use by the sharecroppers, and kept careful business records of costs and output. Furthermore, such farms did introduce modern equipment when profitable, and the managers in the most businesslike manner decided when to buy and use tractors and other equipment that, when purchased, were used on all of the sharecroppers' lands. Under the so-called through-and-through system, managers hired wage workers to run the machinery on the land of the sharecroppers, whether they wanted it or not, and charged them for the service. Finally, ginning, packing, compressing, marketing, financing, and other aspects of the farm business were conducted with the most modern equipment and methods available.

In some areas tenant farms did remain small, sporadically supervised, undercapitalized, and marginally profitable; in these respects they closely resembled farms of small landowners. Attributing conditions in these areas to the efforts of landlords to coerce labor in preference to using modern technology is, however, questionable. The incentive to use machines to replace labor is to cut costs; if using hand labor is cheaper than using machines, machines will not be used. Furthermore, where landholdings are small and where the only labor to be saved is that of the family, the inducement to use machines is low even if the farmer can afford them. Such was usually the case among various kinds of tenants and among small landowners working their own farms. (It should not be forgotten that, despite the growth of tenancy and sharecropping, as late as 1920 at least half of the improved acreage in the South was cultivated by owners.) What was true of tractors was even more true of machinery to pick cotton, which was expensive and required large acreage to be profitable. Plantations cultivated predominantly by sharecroppers were the first in the South to use cotton pickers and then only after a careful calculation of the costs of hand versus machine labor. Farmers elsewhere, particularly those on the hilly lands where tenancy and small landownership were widespread, did not adopt the machines when they became available; mechanization gave these areas no com-

<sup>4</sup> For a detailed discussion of the legal position of the sharecropper, see my "Post-Civil War Southern Agriculture and the Law," *Agricultural History*, 53 (1979): 319-37.



petitive edge against the plantation areas in the South and the new irrigated cotton lands of the Southwest.

I have drawn the line separating the small farm-tenant areas from the plantation-sharecropping regions rather sharply, ignoring the variations that sometimes blurred it. Nevertheless, the distinction was there; significantly, it corresponded to other features of the post-Civil War economy and society. Sharecropping was strongly associated with race: blacks more often became sharecroppers than did whites, who tended to be small landowners and tenants. Sharecropping predominated on the better lands of the delta and in some of the older black-belt plantation areas, and sharecroppers tended to concentrate more on staples, particularly cotton, than did tenants and small landowners.

Racism and labor coercion alone do not explain these patterns; more important is the history of the various classes in the South following the Civil War. The transformations of slaves into free workers and of self-sufficient yeomen into commercial farmers were very different processes that cannot be appreciated unless we distinguish between sharecroppers and tenants. Lumping these two groups together into an undifferentiated class of coerced or "bound" workers ruled by a kind of nonbourgeois "Junker" aristocracy obscures rather than clarifies matters.

OBVIOUSLY, THE DEVELOPMENT OF THE SOUTH after the Civil War did not exactly follow that of the North, and any history of the section must account for differences between them. Professor Wiener, however, first unnecessarily complicates matters by insisting that the South possessed an economic system completely different from that of the North and then oversimplifies matters by describing that system merely in terms of the obstacles to the free market in labor. Nevertheless, his effort to break away from the narrow perspective of cost accounting and income analysis is significant, and I welcome the opportunity to participate in what is a continuing and useful dialogue.

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## Reply:

ROBERT HIGGS TAKES MY DISAGREEMENT with his book as a personal insult; he seems to feel it necessary to defend his “character,” when it is only his interpretation that I have questioned. I do not think I have distorted the work of Higgs, Stephen J. DeCanio, or Joseph D. Reid, Jr., but interested readers will have to decide for themselves. I agree with Harold D. Woodman that the central development of postwar Southern history was the “emergence of new classes and new class relationships.”<sup>1</sup> We disagree about the nature of those relationships between 1865 and 1930, but I have never argued that they were not capitalist.

PROFESSOR HIGGS PORTRAYS HIMSELF AS A SCIENTIST, gathering the facts in a disinterested search for truth, while I “cook up” evidence to fit my preconceptions. In fact, neither of us is “neutral”; each of us offers an interpretation that each tries to support with evidence. Higgs’s evidence is quantitative, but it is still subject to the same criteria of adequacy as nonquantitative evidence: do the data support his interpretation? Does other evidence challenge his interpretation? Does another interpretation account for the evidence in a more satisfactory way? I accepted his evidence concerning the South’s economic growth, his estimate that the average black earned \$43 more in 1900 than in 1867.<sup>2</sup> I questioned his interpretation that this \$43 was an achievement made possible by the existence of a free market economy. I suggested that a class interpretation provides a more adequate understanding and that it permits us to see relationships Higgs has not perceived—among the region’s distinctive class structure, repressive politics, and slow pace of mechanization in agriculture.

Professor Higgs challenges me on six points, ranging from the date on which tractors were introduced to the significance of the debt problem for Populist strategy. A response is difficult because, although he states that he disagrees, he does not explain why. He does raise a substantive objection concerning the nonplantation South: by not taking it into account, I overlooked that area in which black achievements were most impressive. We do, however, have evidence that takes the nonplantation South into account: Higgs’s finding that, between 1867 and 1900, black annual income rose \$43. I have already argued that this increase should not be interpreted as impressive. In Higgs’s view, Southern cities

<sup>1</sup> Woodman, “Sequel to Slavery: The New History Views the Postbellum South,” *Journal of Southern History*, 43 (1977): 552.

<sup>2</sup> Higgs’s evidence has been challenged by Roger Ransom and Richard Sutch, who have shown that the Southern economy stagnated after 1879; Ransom and Sutch, “Growth and Welfare in the American South of the Nineteenth Century,” *Explorations in Economic History*, 16 (1979): Table 4. His evidence has also been challenged implicitly by Joseph D. Reid, Jr., who has argued that the real income gap between Southerners and non-Southerners increased between 1880 and 1900; Reid, “White Land, Black Labor, and Agricultural Stagnation: The Causes and Effects of Sharecropping in the Postbellum South,” *ibid.*, 31.

and industries grew rapidly; I have argued elsewhere that, in comparison to Northern cities, those in the South developed slowly and uncertainly—a consequence of the South's distinctive agrarian class structure.<sup>3</sup> Thus, we disagree about the cities in the same way that we disagree about the countryside.

Professor Higgs criticizes a class interpretation for oversimplifying history, yet he writes that blacks “knew who their enemies were, and the crackers stood at the top of the list.” Is this what he means by “accommodat[ing] . . . the rich diversity of the actual historical experience”? He insists on “exact[ing] empirical procedures,” yet he offers no evidence of any kind to support his view of black attitudes—perhaps because there is virtually none. At other points in his comment, however, he does cite additional evidence in support of his position. A quantitative analysis indicates that 61.9 percent of his citations are to himself. It is undoubtedly true that the strongest support for Higgs's interpretation is found in his own work; that kind of reasoning, however, is usually regarded not as “scientific” but rather as “circular.”<sup>4</sup>

PROFESSOR WOODMAN SEES a slow but steady development of Northern-styled class relations in the South, beginning with Reconstruction; I see the establishment of a qualitatively different kind of capitalism in the immediate postwar period, one in which the dominant planter class denied tenants and sharecroppers many of the formal freedoms held by their Northern counterparts. This system survived until the crisis of the Depression of the 1930s and World War II, which brought a transition to Northern-styled or “classic” capitalism in the South. The distinction between two qualitatively different routes to modern capitalist society was suggested by Barrington Moore's contrast between the English “bourgeois revolution” and the Prussian “revolution from above.” Space restrictions make it impossible to take up Woodman's thoughtful comments on the Moore thesis; I have considered it at length elsewhere.<sup>5</sup>

Professor Woodman's argument that class relations on the plantations were not qualitatively distinct from those on Northern farms rests on several points: planters closely supervised laborers, used advanced methods of production and management, mechanized when profitable, and did not tie laborers to the land. I am willing to concede that planters closely supervised sharecroppers. But their use of advanced methods of production and management would be a more significant indicator of classically capitalist agriculture; and the evidence does not support Woodman on this point. C. Vann Woodward has described the postwar plantation system as “the plantation without the system, . . . the plantation

<sup>3</sup> See my *Social Origins of the New South: Alabama, 1860–1885* (Baton Rouge, 1978), chaps. 5, 6.

<sup>4</sup> I have counted among the self-citations Professor Higgs's citation of discussions of his work by two other scholars. On the significance of self-citation, see my “Footnote—or Perish,” *Dissent*, 21 (1974): 588–92.

<sup>5</sup> See my “Review of Reviews: *Social Origins of Dictatorship and Democracy*,” *History and Theory*, 15 (1976): 146–75. For a similar, though not identical, distinction between the “Prussian” and “classic” routes to capitalist society, see E. J. Hobsbawm, *The Age of Capital, 1848–1875* (London, 1975), 221–22; and, for another effort to apply the Moore thesis to the postwar South, see Dwight B. Billings, Jr., *Planters and the Making of a “New South”: Class, Politics, and Development in North Carolina, 1865–1900* (Chapel Hill, 1979), esp. chap. 4.

minus such scant efficiency, planning, responsible supervision and soil conservation as the old system provided.”<sup>6</sup> British textile manufacturers, who consumed most of the American cotton crop in the early twentieth century, regarded the “archaic and slovenly methods” of American cotton production as the major problem of their industry. An official report of their association examines the “sad state” of American cotton growing in 1907 and criticizes the “ignorant selection of seed, too rapid ginning, poor compresses, and damp in cotton due to overexposure and inadequate storage facilities.” The association attributed these problems to the “vicious circle of tenancy and financing.”<sup>7</sup> This description contrasts sharply with Woodman’s, that “ginning, packing, compressing, marketing, financing . . . were conducted with the most modern equipment and methods available.”

The planters “did introduce modern equipment when profitable,” Professor Woodman writes. This argument avoids the more fundamental problem: to explain why planters did not find it profitable to introduce modern equipment until the mid-twentieth century, several decades after Northern farmers had mechanized. My explanation is that planters kept wages low with a repressive legal system and informal regional practice that limited the freedom of laborers to move out of cotton agriculture. The Depression of the 1930s, with its great labor surplus, led planters to loosen laborers’ bonds; blacks promptly left the South in unprecedented numbers. This migration led eventually to a labor shortage during and after World War II, when, for the first time, laborers were free to demand higher wages. Only at this point did the introduction of modern equipment become profitable.

Professor Woodman considers the great Mississippi Delta plantations to have been classically capitalist enterprises. Yet the differences between them and Northern farms in the late nineteenth and early twentieth centuries were striking. These plantations made extensive use of convict labor—a system created and then monopolized by the great planters. More than a thousand convicts worked Delta plantations at the turn of the century; the conditions under which they labored made slavery, in the words of Albert D. Kirwan, look like “a mild and humane institution.”<sup>8</sup> The rest of the Delta plantation laborers were, of course, “free” blacks compelled by the region’s practices to submit to intensive exploitation. Delta planters did not permit qualified laborers to rent land, much less buy it; all were required to sharecrop, which was more profitable for planters. They shrunk the size of the sharecroppers’ plots down to an average of twenty-three acres in 1910, compelling croppers to cultivate more intensively, but without advanced techniques or mechanization. In these respects, class relations on the postwar plantation were qualitatively distinct from those that prevailed on Northern farms during the same period.

<sup>6</sup> Woodward, *Origins of the New South, 1877-1913* (Baton Rouge, 1951), 179.

<sup>7</sup> International Congress of Delegated Representatives of Master Cotton Spinners and Manufacturers’ Association, *Official Report, 1907* (Manchester, England, 1907), as quoted in Robert L. Brandfon, *Cotton Kingdom of the New South: A History of the Yazoo Mississippi Delta from Reconstruction to the Twentieth Century* (Cambridge, Mass., 1967), 120.

<sup>8</sup> Kirwan, *Revolt of the Rednecks: Mississippi Politics, 1876-1925* (New York, 1951), 158-59.

SOUTHERN SHARECROPPERS COULD NOT HAVE BEEN BOUND by debt peonage, Professor Woodman claims, because they moved frequently. Let us admit that we lack adequate evidence concerning the frequency with which Southern tenants and sharecroppers moved.<sup>9</sup> But the extent of debt peonage cannot be determined by gathering better data on the geographical mobility of sharecroppers. I must repeat that peonage may exist even when tenants move among landlords. According to regional practice, an indebted tenant could move only with his landlord's permission and only when his landlord sold his debt to the new landlord. As long as the debt moved with the tenant, the coercive aspect of the system remained.<sup>10</sup> The real question, then, is not how many tenants moved but, rather, how many were unable to pay their debt at the end of season, when it was due. When Woodman argues that Southern blacks moved west after the Civil War, he overlooks evidence that the South's distinctive laws eventually restricted this migration. Robert L. Brandfon's study of the Mississippi Delta plantations reports that the migration of blacks into Mississippi "ended" when the states to the east established legal restrictions on labor recruiting in the 1870s; by 1880 Delta planters recruited laborers primarily from adjacent counties within Mississippi.<sup>11</sup> Later, when Texas and Arkansas planters tried to recruit blacks in Mississippi, planters there responded in the same way that their counterparts in the Southeast had; they passed resolutions that promised to "tar and feather" anyone caught "endeavoring to decoy away" their laborers.<sup>12</sup>

Professor Woodman correctly insists that we must distinguish the class position of white tenants in the uplands from that of black sharecroppers. I have argued similarly elsewhere against the view that a single merchant-planter class dominated both groups. The antebellum planter class became "planter-merchants" in the black belt, while in the uplands a new class of "merchant-landlords" arose after the war. Whatever their structural similarities, the two groups remained socially distinct and politically antagonistic on some key issues throughout the 1880s.<sup>13</sup> But it is not so clear that upland whites worked on small farms as owners or tenants, rather than on plantations, as Woodman suggests. Reliable data on this question is hard to find, because the Census Bureau did not take up the issue until 1910. In a special study, it defined the plantation as a continuous landholding with a single owner, divided into five or more smaller tracts that tenants leased. Using this definition, the Bureau found that

<sup>9</sup> Professor Woodman's citation of a single study of conditions in 1920 is inadequate for this purpose. The census report on "internal migration" measures change in the total population of each state over a decade, subtracts the number of deaths, and defines the resulting figure as "net migration." According to this definition, an Alabama sharecropper who moved five miles across the Mississippi border to work land owned by the same planter would be a "migrant," while his neighbor who left sharecropping and moved one hundred and fifty miles to Birmingham to work in the steel mills would not—even though the latter move would be much more significant for our purposes. U.S. Bureau of the Census, *Historical Statistics of the United States: Colonial Times to 1970*, 1 (Washington, 1975): 93–95.

<sup>10</sup> Daniel A. Novak, *The Wheel of Servitude: Black Forced Labor after Slavery* (Lexington, Ky., 1978), 48, 51, 63, 73–74, 76–77; and Hortense Powdermaker, *After Freedom: A Cultural Study of the Deep South* (New York, 1939), 86–89.

<sup>11</sup> Brandfon, *Cotton Kingdom of the New South*, 136.

<sup>12</sup> Raymond (Miss.) *Gazette*, December 18, 1886, as quoted in Vernon L. Wharton, *The Negro in Mississippi, 1865–1900* (Chapel Hill, 1947), 111.

<sup>13</sup> See my *Social Origins of the New South*, 92–93.

by the turn of the century the plantation system had spread well beyond the black belts into the largely white upland regions. In Alabama, for instance, where the black belt included one-third of the counties, the "plantation region" had spread to almost three-quarters of them.<sup>14</sup>

The fate of the Freedmen's Bureau is directly relevant to Professor Woodman's interpretation. The radicals in the bureau understood with remarkable clarity that the North's classic capitalism had its basis in a free labor market, as Eric Foner has argued.<sup>15</sup> Officially, the bureau sought to bring this freedom to Southern blacks—to replace the "lash of slavery" with the "lash of the market." Planters opposed the bureau's free labor policy and worked to re-establish plantation agriculture on a more directly repressive basis. Woodman argues that a free labor market was, in fact, created; yet the Freedmen's Bureau lost its struggle with the planters, and it was disbanded. What, then, was the blacks' position in the postwar Southern economy, given the defeat of the bureau's efforts to establish a free labor market? The planters, in my view, succeeded in making blacks into a kind of bound or unfree labor force.<sup>16</sup>

Whatever my disagreements with Professor Woodman's interpretation, my intellectual debt to him is great. Over the past decade, he has systematically shown the limitations of the neoclassical economic approach to postwar Southern history and pointed the way to a class analysis. He has done this lucidly, yet with modesty. His contribution has been fundamental, not just for my work, but for the entire field of Southern history.

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<sup>14</sup> U.S. Bureau of the Census, *Plantation Farming in the United States* (Washington, 1916), app. 1.

<sup>15</sup> Foner, "The Meaning of Emancipation and the Definition of Freedom," paper delivered at the Conference on the First and Second Reconstructions, University of Missouri, St. Louis, February 15–17, 1978, pp. 6–10, and *Free Soil, Free Labor, Free Men: The Ideology of the Republican Party before the Civil War* (New York, 1970), 11–72. On the roots of this ideology, see Joyce Appleby, "Ideology and Theory: The Tension between Political and Economic Liberalism in Seventeenth-Century England," *AHR*, 81 (1976): 499–515.

<sup>16</sup> This view is consistent with Ira Berlin's argument that the position of blacks in the postwar South was closer to that of the antebellum free black caste than to Northern workers, particularly in the extent to which debt peonage restricted their freedom; see Berlin, *Slaves without Masters: The Free Negro in the Antebellum South* (New York, 1974), xiv–xv, 225–25, 381–82.



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## Review Article

# German History—in the Grand Manner

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GORDON A. CRAIG. *Germany, 1806–1945*. (Oxford History of Modern Europe.) New York: Oxford University Press, 1978. Pp. xv, 825. \$19.95.

JAMES J. SHEEHAN. *German Liberalism in the Nineteenth Century*. Chicago: University of Chicago Press, 1978. Pp. 411. \$27.00.

FRITZ STERN. *Gold and Iron: Bismarck, Bleichröder, and the Building of the German Empire*. New York: Alfred A. Knopf, 1977. Pp. xxiv, 620. \$17.95.

HANS-ULRICH WEHLER. *Das deutsche Kaiserreich, 1871–1918*. Göttingen: Vandenhoeck and Ruprecht, 1973. Pp. 272.

ONE OF THE PARADOXES OF OUR DISCIPLINE is that politics should be the persistent focus of the historical enterprise in Germany, the traditional locus of a non-political culture. Historical writing about Germany, whether of native or of foreign vintage, has centered on the state even at those times when large sections of German society were turning away from its political institutions in deliberate alienation or were accepting benefits from them with studied indifference. The paradox is obvious enough in the twentieth century, when the course of German statecraft has involved the world in a series of disasters culminating in the centripetal Nazi experience and has attracted the attention of historians to the contradictions of a nonpolitical politics. But its essentiality to German history is demonstrated by the political conditioning of German historiography during the nineteenth century. Historians both of the “Prussian school,” who were notoriously engaged, and of the Rankean persuasion, who were putatively detached, showed a political concern that contrasted strangely with the apolitical attitudes of the bourgeois groups that they described and represented. This odd coupling, be it noted, occurred well before the onset of the overt twentieth-century crisis brought the German state into the foreground of historical consideration.

These reflections are occasioned by the recent appearance of the four historical works here under review. Although they cover the spectrum from the tradi-

tional to the innovative approaches to German history, all four continue the main line of German historiography, which places the state at the center of general history. At a time when, both in this country and in Germany, an ineluctable tendency toward the privatization of life and a real abhorrence of politics dominate the national scene, the concentration of historians on the state is indeed noteworthy, and it serves as a constant point of reference through the variegated perspectives on the recent German past. This concentration is, of course, not exclusive, as the recent contributions to economic history and the social cynosures of Wolfram Fischer, Jürgen Kocka, and Rudolf Bauer have made clear; but, aside from the obviously subordinate status of such history (a status literally confirmed by at least one of its propagandists), these exemplars have not been entirely able to escape the ubiquitous magnetism of the German state—or, more precisely, the German states—in history.<sup>1</sup> Thus for Fischer economics and politics have been two equivalent branches of German activity, and he has inquired into both—investigating the state's economic policy and its role, alongside private enterprise, in early industrialization. For Kocka politics has often played the role of the invisible guest; his studies both of the lower middle-class employees of the Siemens company and of German class society in the First World War put social history in the service of unspoken political purposes.

The leading protagonist of the school opposing a contemporary “history of society” (*Gesellschaftsgeschichte*) to the traditional “history of states” (*Staatengeschichte*)—Hans-Ulrich Wehler, author of one of the works under review here—

<sup>1</sup> For Fischer's economic and social history, especially see his early *Handwerksrecht und Handwerkswirtschaft um 1800: Studien zur Sozial- und Wirtschaftsverfassung vor der industriellen Revolution* (Berlin, 1955), the subsequent anthology he edited under the revealing title *Wirtschafts- und sozialgeschichtliche Probleme der frühen Industrialisierung* (Berlin, 1963), and his own later *Wirtschaft und Gesellschaft im Zeitalter der Industrialisierung* (Göttingen, 1972); for Kocka, see his magnum opus entitled *Unternehmensverwaltung und Angestelltenschaft am Beispiel Siemens, 1847–1914: Zum Verhältnis von Kapitalismus und Bürokratie in der deutschen Industrialisierung* (Stuttgart, 1969), and his recent *Unternehmer in der deutschen Industrialisierung* (Göttingen, 1975); and, for Braun, see his *Sozialer und kultureller Wandel in einem ländlichen Industriegebiet im 19. und 20. Jahrhundert* (Stuttgart, 1965), his *Industrialisierung und Volksleben* (Zurich, 1960), and his edition of *Gesellschaft in der industriellen Revolution* (Cologne, 1973). For the involvement of this group with political history, see Fischer's *Der Staat und die Anfänge der Industrialisierung in Baden* (Berlin, 1962), a projected two-volume study of which only volume one has been published, and his *Deutsche Wirtschaftspolitik, 1918–1945* (3d ed., Opladen, 1968); and Kocka's *Klassengesellschaft im Krieg: Deutsche Sozialgeschichte, 1914–1918* (Göttingen, 1973). I omit here the obvious instances of political-social convergence such as has been exemplified by Heinrich August Winkler's analyses of fascism and of the relations between the lower middle classes and the state, by Reinhard Koselleck's masterly interpretation of the Prussian bureaucracy as a social group, and by studies of pressure groups from Thomas Nipperdey, Hartmut Kaelble, and Hans-Jürgen Puhle; see Winkler, *Revolution, Staat, Faschismus* (Göttingen, 1978), and *Mittelstand, Demokratie, und Nationalsozialismus: Die politische Entwicklung von Handwerk und Kleinhandel in der Weimarer Republik* (Cologne, 1972); and Koselleck, *Preussen zwischen Reform und Revolution: Allgemeines Landrecht, Verwaltung, und soziale Bewegung von 1791 bis 1848* (Stuttgart, 1967). For a typical complaint about the weakness of economic and social history in German historiography, see Wolfram Fischer's introduction to his *Wirtschaft und Gesellschaft im Zeitalter der Industrialisierung*, 9–10. It must be admitted that at least one partisan of the traditional political history has claimed that his kind of history has become peripheral in contemporary German historiography and that at least one partisan of the new “history of society” has claimed that a change of “paradigms” has ensconced this kind of history in a newly dominant position. See Andreas Hillgruber, “Politische Geschichte in moderner Sicht,” *Historische Zeitschrift*, 216 (1973): 529–31, 543, 546; and Hans-Ulrich Wehler, “Staatsgeschichte oder Gesellschaftsgeschichte?” in Helmut Berding et al., eds., *Vom Staat des Ancien Regime zum modernen Parteienstaat: Festschrift für Theodor Schieder* (Munich, 1978), 349, and “Moderne Politikgeschichte oder ‘Grosse Politik der Kabinett?’” *Geschichte und Gesellschaft*, 1 (1975): 347–48.

has indeed characterized his misleadingly distinctive label, “history of society,” simply as a “paradigm” for an integrated history that includes “modern political history” (*Politikgeschichte*). In his search for a unifying process through social history, he has explicitly accepted a blend of historical modernization theory and historical materialism as the nominal basis of such a unity. But in his historical work he has tended to deny the validity (in this respect) of “social history” (*Sozialgeschichte*) as an overly specialized discipline and to depict socially grounded political conflicts as the ingredients of this past unity.<sup>2</sup> One feature comes through clearly in this otherwise obscure discussion between antagonists of the differing political and social emphases in historiography: the overlap, characteristic of Germany as it has not been of the United States, between academic and political radicalism among historians. This overlap gives academic confrontations in Germany between traditional historians of states and newer historians of society the character of a political struggle, a combination that has not been evident in the United States. Here, historians of Germany have differing political points of view on the German past because of their partisan political principles, not because of any differences in their academic approach to history. The overlap of political and academic radicalism in Germany explains Wehler’s advocacy of older German historians like Eckhart Kehr, Veit Valentin, and Ludwig Quidde, whose relationship to the “history of society” was peripheral but whose politics were democratic; such historians were, for that reason, “outsiders” with respect to the German historical profession as an institution.<sup>3</sup>

THE FOUR BOOKS UNDER REVIEW differ in their academic heritage, insofar as they range from Gordon A. Craig’s emphasis upon traditional political history through Fritz Stern’s monumental synthesis of politics and society to James J. Sheehan’s and Hans-Ulrich Wehler’s social analyses of political institutions, but they are informed by a common critical perspective with definitely political overtones. All four works take as their leading point of view the political disasters to which German regimes have been subject during our own century. In his introduction Craig forecasts the “tragic story” he has to tell; Fritz Stern conscientiously admits that, despite his attention to contemporaneity, his story “is overladen with the tragedy of latent developments,” so that “it is a record of men sowing the wind, not knowing that a later generation would reap the whirlwind”; Sheehan concludes his discussion of the relationship between Nazism and German liberalism with the climactic assertion that “the liberal constituency proved to be especially susceptible to Nazism after 1930”; and Wehler candidly concedes that his repair of the previously neglected “necessary synchronization of socioeconomic and political development . . . in the empire” is

<sup>2</sup> Wehler, “Vorüberlegungen zu einer modernen deutschen Gesellschaftsgeschichte,” in his *Industrielle Gesellschaft und politisches System* (Bonn, 1978), 3–20.

<sup>3</sup> Wehler, “Staatsgeschichte oder Gesellschaftsgeschichte?” 350–68.

overweighted on the side of “the system of political domination.”<sup>4</sup> From this teleological standpoint the authors write with the optimal blend of sympathy and criticism that the historiography of our time has deemed the best way of understanding a past that both existed in its own right and has become constituent in a problematical present.

Fritz Stern’s work, *Blood and Iron*, stands out, in this galaxy of fundamental books on modern German history, for its successful juncture of political and academic categories. It combines contemporaneity and teleology, periodic discontinuity and a sense of the periodic connections within the recent German past; and it focuses, too, on authoritative personalities as the natural meeting-points of political and social history, of traditional and innovative historiography. Stern’s choice of Otto von Bismarck, Gerson Bleichröder, and the relations between the two finesses the parlous issue of representative individuals, and his selection of persons who both roamed the corridors of political power and were viewed by their contemporaries as quintessential leaders of their respective social groups—that is, of the Prussian Junker landowners and of the assimilated, well-to-do Jewry—enables him to recount their story in the full confidence that what he finds happening to them is relevant to both state and society. Bleichröder was a Berlin financier and probably the most successful private banker of his time. He was deeply involved in the financing of many major enterprises in the era of Germany’s rapid industrialization and in financing the Prussian and German governments in both peace and war. But he was also Bismarck’s personal banker, his adviser in matters of public finance, and a frequent tool of his foreign policy. In these two men the old Prussian establishment and the new upper bourgeoisie of financial capitalism came together in an uneasy, but mutually profitable, working relationship.

What Bismarck did was obviously important for the Prussian and German states, and what Bleichröder did was just as obviously important for the private world of German finance and for the social status of German Jewry. Thus, the relationship between Bismarck and Bleichröder, as Stern narrates and analyzes it, overtly personifies the relations between the German states and society (or at least the financial species thereof) in the second half of the nineteenth century. Stern’s account, which blends the secondary literature with extensive archival research, reveals the cautious, and heretofore unanticipated, concern of the Prussian and German governments with the financial dimension of their daring policies and documents the heretofore suspected ambitions of German economic magnates—that is, the bankers—for the trappings of political power.

Stern exposes deeper-lying veins as well: the private, social side of Bismarck and the public, political side of Bleichröder. In so doing Stern reaches some surprising conclusions on the untroubled mixing of private and public interests in

<sup>4</sup> Craig, *Germany, 1806–1945*, vii; Stern, *Blood and Iron*, xxiii–xxiv; Sheehan, *German Liberalism in the Nineteenth Century*, 281–83; and Wehler, *Das deutsche Kaiserreich*, 13–17. All succeeding references in the text to the books here under review will not carry citations or page numbers in order to avoid cluttering the notes with a series of *ibid*’s and the text with long numerical parentheses.

Bismarck's activities and on the troubled mixing of political and social concerns in Bleichröder's representation of Jewish interests. Because Bleichröder played his role as an agent of social modernization in a country whose historiography has been overwhelmingly political and intellectual and because he was a prominent Jew in a period when the new anti-Semitism was raising its ugly head for the first but unfortunately not the last time, Bleichröder, in Stern's memorable expression, was "everything that has been left out of German history." Stern has been able to fill this gap especially by exploiting the personal and business records of Bleichröder.

But Stern also adds to our already considerable knowledge of the great Bismarck and of such lesser public luminaries as his son Herbert, his sometime subordinate Friedrich von Holstein, and his ambassadors Harry von Arnim, Georg von Münster, and Paul von Hatzfeldt by showing them in their financial and sectarian relations with Bleichröder. These previously unnoticed *aperçus* are dug out of archival treasures, both Bleichrödian and Bismarckian. However we judge their representativeness, these luminaries and their ilk deemed Bismarck and Bleichröder representative of their respective social groups as well as idiosyncratically dominant in Prussian and German politics, and, insofar as they thought so, their judgments invite impressionistic rather than quantitative analysis. And this is what we get from Stern.

From Stern's rich knowledge of and feeling for history in general and for the Bismarckian period, its politics, and its social attitudes in particular, we also get explanatory causal lines of great significance: from history in general, propositions on the unconventional character of revolutions, on mystique as a desirable concomitant of political power, on the "universal effort to legitimize new wealth," on the discrepancy between code and conduct "in all human affairs," on the stock market's abhorrence of uncertainty "as nature abhors a vacuum"; and, from German history during the imperial period in particular, contextual judgments, descriptions, and analyses in elaboration of the Prussian and German economic and diplomatic policies crystallized by Bismarck, of German aristocratic attitudes aimed against Bismarck, of German economic and Jewish social developments crystallized by Bleichröder (Bleichröder "was part of the great transformation of German society"), and of the German social attitudes composing the new anti-Semitism, which concentrated against Bleichröder as its leading symbol. Thus, Stern especially stresses the linkages between German domestic and foreign policy through Bismarck and the ambiguous attitude toward the new money on the part of the traditional aristocracy that was particularly embodied in its dual orientation toward Bleichröder—general themes that are important for German political and social history and that manifest the balance between the autonomous discontinuity and the teleological continuity in the recent German past, a historiographical balance at which Stern excels.

Gordon A. Craig's magisterial study of Germany during the period of its unification, *Germany, 1806–1945*, may stand as the definitive statement on modern German history by his passing generation, the generation that was politically

educated by the totalitarian process that ended a united Germany. It features the most impressive qualities of this generation's historiography: the frank address to politics as the most crucial activity of man's past; the conscientious devotion to the truth of the facts; a critical approach both to the object and to the sources of history; the courage to face the fundamental questions of national history; and the discrepancy between the substantive belief in indeterminism and the historical equality of all peoples in all their ages, on the one hand, and the formal belief in historical continuity, on the other.

Certainly, Craig's focus on politics is not exclusive: there are sections on Germany's economic and social development, on German culture in the narrow sense both high and low and both before and after the First World War, and on German women both before and during the Nazi period. The pages on German popular culture—including education—and on the successive stages in the career of German women are especially notable, largely because these subjects are inherently difficult to cover in a coherent survey and because previous attempts to study them have been so specialized and so inadequate as to make Craig's considerations a genuine contribution. And yet not only does the bulk of Craig's work recount and characterize the detailed activities of Germany's governing and administering elite, but the tone of Craig's treatment, when he gets to diplomacy and to the preparations and the deeds of the military, takes on a warmth and an urgency that is lacking from all of his other topics, with the exception of the novel, another of Craig's long-established commitments. Even when he handles the nonpolitical aspects of Germany's recent past, moreover, Craig's preference is ever for the political relationships of the social, economic, and cultural aspects of German life: thus, social and economic developments are described particularly in the context of German economic policy, while German cultural history is repeatedly discussed in relationship to the political indifference of the German literati.

Craig's address to the facts he adduces is exemplary. Not only does he clarify and dramatize them by the understanding character sketch, the telling episode, or the apt quotation (Craig's intimate knowledge of German drama, poetry, and the novel here stands him in good stead, both for the useful, contextual chapter headings and for the revealing briefer references that are scattered through the text), but he faces squarely the outstanding factual dilemmas within modern German society. Why did Bismarck change course so radically in the years 1878–79? What made him adopt a procolonialist stance in 1884? How serious was his interest in social policy? What was behind his forced resignation in 1890? What was the role of the emperor William II in the fateful course of German policy that led that nation into World War I? What was the German responsibility for that conflict? What kind of “revolution” was the German revolution of 1918 and, in general, what were the ingredients of “the troubled life” and what accounts for the “failure” of the Weimar Republic? Who was accountable for the providential Reichstag fire of February 1933? What was behind Hitler's appeal in the Nazi rise to power? What was the contribution of



the Four-Year Plan to German economic development? Was the German working class affected favorably or unfavorably by Nazi policies? What effect, if any, did the course of the Second World War have on the Nazi regime? To all of these questions, so important in modern German history, Craig gives a decisive answer, either advancing the relevant material for it or judiciously correcting opinions that he finds insufficiently supported.

At its best Craig's continual concern with the connection between society and the German state affords an integrative thread through the whole of the variegated modern German past. At the very least it manifests a critical attitude that lends a formal cohesion to his history. Therefore, German social and economic developments were crystallized into pressure groups that evinced "self-interest" and "greed," while the unpolitical German poets left "the great issues of society and politics to those in authority." This kind of material thread and attitudinal cohesion are all the more valuable for Craig's overt subscription to "the play of chance and personality" in history. Time and time again Craig makes this point, both abstractly and operationally. Thus, in his preface he promises to present "courage and steadfastness," "devotion to the cause of liberty," and "resistance to the evils of tyranny" as well as what has been dark in the history of united Germany; he self-consciously depicts the process of German unification in terms of Bismarck's personality and politics, despite the current fashion that attributes "primary importance" to "economic and social forces in history"; he explicitly advocates individuality and rejects determinism in his introduction of Wilhelminian diplomacy; and, perhaps most fundamentally, he argues for Hitler's ahistorical discontinuity and indicts historians who believe in the continuity of German history from Bismarck to Hitler as "wrong." In short, Craig faces up to the basic questions in modern German history—notably, the questions concerning the relations of domestic and foreign policy and concerning the continuity of the recent German past in the persistently prominent role it has had in the succession of global disasters during our century—and answers them as an autonomist in principle and as an integrationist in practice.

James J. Sheehan paints his history of *German Liberalism in the Nineteenth Century* on a broad canvas—that is, essentially from the pre-1848 period beginning in 1815 (nominally from the origins of the movement around 1770) until the end of the peace-time Wilhelminian regime in 1914. Hence, his study has to be entered in the list of general German histories; and, appropriately for a member of the current historiographical generation, Sheehan makes his contribution primarily in the realm of social history. The three distinctive dimensions of Sheehan's material and analysis, dimensions informing *seriatim* each of the six well-known and successive periods in which he articulates his account, all fall roughly into the categories of political sociology familiar from his previous research. Sheehan reveals better than anyone before him, first, the impact of local and regional diversity upon generic liberalism in Germany; second, the social relationship, at first convergent but increasingly divergent with time, between the liberal leadership and its electoral clientele; and, third, the social composi-

tion of both notables and followers in the liberal camp. In the second category Sheehan provides a valuable but unsurprising demonstration of the growing difference between the leadership, which was increasingly composed of notables formed by property (*Besitz*) and higher education (*Bildung*), and followers, who were increasingly composed of those who should now be classified as lower middle class (although Sheehan is himself much too egalitarian so to classify them). Also noteworthy in this category is Sheehan's revelation of the persistent social fear of the working masses on the part of the liberal leaders. In the third category Sheehan assembles very useful, but again hardly surprising, statistics on liberalism's rise under conditions of limited suffrage and subsequent decline under conditions of democratization. But here Sheehan also reveals the changing social fortunes of the middling groups and the changing meaning of the term *Mittelstand*, used to signify the middle strata of German society upon which the liberals pinned their moral and their political hopes. Given this emphasis, Sheehan quite naturally gives special thanks in his preface to Hans-Ulrich Wehler, the German doyen of the new social history.

Yet Sheehan's goal is more general than this collection and processing of the social dimensions in German liberalism. It is nothing less than to explain the liberals' failure as such—that is, “to attempt a synthesis which would describe how German liberalism looked when all of its components were seen together.” He anticipates his particular social emphasis by announcing his concern with “the apparently necessary evolution of long-term trends” and with “the way in which the historical situation narrowed liberals' choices” rather than with the “moral deficiencies” and “the specific mistakes” of the German liberals. But his repeated references to the persistence of the liberals' ideas in their traditional postures, to their internal divisions, to the “ambivalence” of the liberal relationship to “the state” (*der Staat*) as well as to “the people” (*das Volk*), and to the vagueness in the liberals' programmatic thinking betoken an inclusion of optional political facets in his consideration, which are more recognizable and hence less profitable than the originality of his social treatment. It is hardly surprising, then, to find that Sheehan concludes with an analysis of the Nazi appeal to the “Protestant middle strata” that had constituted the liberal electorate in the nineteenth century, an analysis that, despite his nominal protestation against the implication that the triumph of Nazism meant “the culmination of German history,” does indicate Sheehan's overriding (and gratifying) concern with the political ramifications of the German social development toward modernity.

If the political adjunct is deemed inevitable in a subject like liberalism and in an American historian of Germany, the same necessity may not, at first glance, seem applicable to a general history of imperial Germany by a socially oriented, authentically German historian. Yet the political reference is at least as characteristic of this work, *Das deutsche Kaiserreich*, and of this historian, Hans-Ulrich Wehler, as it is of the history of a political movement by a foreign scholar like Sheehan. Both of the elaborate, hostile discussions that the book has evoked in Germany—the review essays by Hans-Günter Zmarzlik in the pages of the *His-*

*torische Zeitschrift* and by Thomas Nipperdey in Wehler's own *Geschichte und Gesellschaft*—have stressed the anomalous political emphasis in Wehler's self-consciously social approach. Nipperdey, indeed, has gone so far as to label Wehler a “Treitschke *redivivus*” by reason of a “passion and partisanship” that refers to Wehler's critical political attitude toward the history of imperial Germany.<sup>5</sup> Wehler himself admits in a postscript to *Das deutsche Kaiserreich* that the work is more politically centered and has less on “social structure and social development” than he originally envisaged, and he attributes this imbalance to circumstantial factors such as the use of teaching notes (for both lectures and seminars) as the basis of the work, the requirements stemming from its status as an entry in a general series on German history, and the paucity of the necessary empirical monographs that could serve as the foundation for a social survey as well as to more fundamental theoretical considerations.<sup>6</sup>

But these considerations entail a political point of view more essentially than their overt orientation toward the problematical analysis of social classes and the equally problematical comparative approach to economic and social modernization theory would seem to indicate.<sup>7</sup> For class structure, like economic and social growth, is a social concept designed to furnish the historical integration traditionally supplied by the state. The problems associated with such concepts—for example, the spontaneous behavior of groups in Western countries that fail to follow the standard pattern of economic and social growth—render these concepts incapable of filling an integrative function in the German case without the inclusion of an autonomous political dimension. Thus, Wehler's constant theme, which furnishes him with a persistent canon of interpretation in *Das deutsche Kaiserreich*, is the amalgamation of a reactionary class of landed aristocrats, who “partially adapted themselves to modernization,” and an adaptable class of feudalized bourgeois, who represented “large-scale industry” in the process of modernization, into a “leadership elite,” whose common interests were to dominate Germany and to limit the democratic possibilities of modernization through a combination of repression and diversion. Wehler's approach is, therefore, primarily functional. After an initial brief discussion of German economic and social development in the integral combination of “organized capitalism and the interventionist state,” the bulk of the work articulates the various components of “the system of dominion and politics”—to wit, the governing system, which includes politics proper; the interest groups; ideology, including nationalism and anti-Semitism; religion; the institutions of socialization, like the family and the schools; the judicial system; social policy; tax and financial policy; armaments policy and the military, imperialism, foreign policy, and war

<sup>5</sup> Zmarzlik, “Das Kaiserreich in neuen Sicht?” *Historische Zeitschrift*, 222 (1976): 105–26, esp. 109; and Nipperdey, “Wehler's ‘Kaiserreich’: Eine kritische Auseinandersetzung,” *Geschichte und Gesellschaft*, 1 (1975): 539–60, esp. 542, 560.

<sup>6</sup> In addition to his postscript in *Das deutsche Kaiserreich*, see his principled response to his critics in Wehler, “Kritik und kritische Antikritik,” *Historische Zeitschrift*, 225 (1977): 347–84, esp. 348.

<sup>7</sup> See Wehler's insistence that Marx be supplemented by Max Weber in this context; “Vorüberlegungen zu einer modernen deutschen Gesellschaftsgeschichte,” 3–4, 15–20.

(that is, the First World War)—and is interpreted homogeneously in political terms. Social policy, for example, is classified under the label of “compensation for the assurance of loyalty.” The most successful facet of this unremitting application of what Wehler calls the “problem-oriented historical structural analysis of German society and its politics” is the linkage of foreign and domestic policy by subjecting both to the requirements of the oligarchically dominated hierarchical society, a linkage for which Wehler finds more deliberate supporting evidence than is usual for such historiographically dictated correlations.

Sequential development within the period of the Second Empire is obviously subordinate in Wehler’s thinking to the persistent structures of the whole age from 1871 to 1918. But intraperiodic change is not totally absent. The Bismarckian era (1862–90) is a “Bonapartist-type dictatorial regime,” signifying an early industrial kind of authoritarian rule in which personal dominion is joined to the charismatic blend of progressive concessions and conservative stabilization. This era was followed, in Wehler’s account, by the “persistent crisis of the state” in the post-Bismarckian era (that is, from 1890 to 1918), subtitled “authoritarian polyarchy without coordination” to signify the plural roots of a decapitated regime. These two eras were themselves actually articulated into two overlapping phases of “negative integration.” The first of the phases, the period from 1871 to 1890, was characterized by a conservative “politics of coalition.” The second phase was the period from 1876 to 1918, in which the nominal multiplicity of power centers in the imperial age was really dominated by traditional oligarchs in the effort to stem the advancing democratic tide.

Clearly, Wehler’s stages of development within the imperial age are of comparatively little interest vis-à-vis the overall interpretation of the age in terms of a distinctive kind of German modernization that restricted the egalitarian consequences of social growth through a political alliance of authoritarian power elites; clearly too, this constancy of sociopolitical factors is linked to a principled belief in the continuity of modern German history between the beginning of the Second Empire and the end of the Third (or Nazi) Empire, a belief that lies behind Wehler’s subscription to “critical history.” Thus, though opposing on empirical grounds Fritz Fischer’s thesis that a deliberately bellicose policy on the part of the Wilhelminian civil and military authorities led to World War I, Wehler does endorse the larger Fischer thesis of persistence in modern German history. Wehler obviously abhors the persistent authoritarianism in this history, and, if he finds its roots in Germany’s hybrid social structure rather than in Fischer’s political documents, his interpretation is still dominated by his opposition to what in his view was the continuous course of German politics that had its climax in 1945.

SOME YEARS AGO GEOFFREY BARRACLOUGH called for a new approach to modern German history, an approach that would dispense with the “liberal” revulsion against the conservative and neoconservative German politics of the first half-

century and decentralize the Nazi experience in the interpretation of Germany in our own era.<sup>8</sup> It is good to find that the older liberal and critical approach can still deliver works that are intellectually interesting and empirically inventive. This approach has obviously proved to be capable of absorbing the newer social materials and concepts in Western historiography while continuing to demonstrate the critical and documentary method that was designed to discover and elucidate discrete acts of state and church. Not so obvious is the way in which the combination of absorption and persistence has been accomplished, and here the works under consideration furnish object lessons. Social evidence, both qualitative and statistical, is subsumed under such relatively intelligible processes as industrialization, modernization, or economic growth. Declinations from these organizing concepts, such as those that occurred in modern German history, require that the maverick facts, social and political, and the flawed social process be integrated into a general political process, which can then serve as the unifying thread of historical interpretation.

The states in Germany, both national and particular, have remained the explicit or implicit themes of modern German history because historians have been primarily interested in the constancy behind the diverse claims that the social agents of those states have made upon them. Thematic history is thus particularly appropriate to modern Germany. As the objects of thematic history, the states have been treated as the threads of continuity through the modern German past, because their ever-changing traditions have actually represented the balance between spontaneity and continuity. The pervasive view of the Nazi regime as the culmination of German political authoritarianism is an expression of this thematic continuity and does not obviate the consideration of each period in terms of its spontaneity as well. Just as the analysis of social process as such has been the hallmark of recent French historiography and the multiple commitment to a sophisticated Marxism, historical demography, and a wide-ranging social empiricism has been the hallmark of recent British historiography, a sort of economic and sociological politics is the pre-eminent characteristic in the contemporary historiography of modern Germany.<sup>9</sup>

The quality of historiography takes its character from the nature of the history under consideration. The nature of modern German history is such that the integration of social forms into political structure and governmental policy is as much a historical fact as a historiographical necessity.

<sup>8</sup> Barraclough, Review: part 1, "Mandarins and Nazis"; part 2, "The Liberals and German History"; and part 3, "A New View of German History," in the *New York Review of Books*, October 19, 1972, pp. 37–43; November 2, 1972, pp. 32–38; and November 16, 1972, pp. 25–31.

<sup>9</sup> See Georg G. Iggers, *New Directions in European Historiography* (Middletown, Conn., 1975), *passim*, esp. 80–122.

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## Reviews of Books

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### GENERAL

REINHARD BENDIX. *Kings or People: Power and the Mandate to Rule*. Berkeley and Los Angeles: University of California Press. 1978. Pp. xii, 692. \$20.00.

Reinhard Bendix has written a major comparative account of the history of five countries, seeking to determine which factors made possible the rise of royal authority in the early period of their development and which factors accounted for the emergence more recently of political practices and institutions legitimized by the will of the people. The case studies are England, France, Germany, Russia, and Japan, with occasional excursions on China and the world of Islam. The author has conducted his inquiry with skill, intelligence, and vast learning. Here and there the reader will hear echoes and overtones of Arnold Toynbee and William H. McNeill, less in the conclusions that are reached than in the questions asked and the techniques employed. This is comparative history in the grand tradition, bold, challenging, scholarly, and ingenious.

The five main themes advanced in the book are, first, that the authority of kings depended historically not only on internal and external struggles for power but even more on religious sanctions that provided it with ultimate justification. Second, kings governed their realms with the aid of noblemen or notables to whom they delegated authority but from whom they also expected obedience. The tension between central and local government had to be continually managed yet could never be resolved. Third, authority in the name of the people gradually became an alternative to authority in the name of the king, as the commercialization of land and public office as well as the increasing role of educated commoners in high places undermined the position of the crown. Fourth, although the transition from royal to popular sovereignty occurred in every country, the process varied greatly from one to the other, so that each model reflects

differences in its concrete historical experiences. Finally, the spread of government by popular mandate has been a continuous movement, because the more backward countries have been encouraged by the example of the more advanced ones, particularly England, France, and America, to undertake the reconstruction of authority within their own borders. The first and the last of these five points are of special importance—that is, the author is most insistent on the role of religion as the foundation of royal power and on the force of precedent as inspiring the transition to popular sovereignty.

The book rests on a solid mastery of the historical literature, analyzed and interpreted by a sharp mind. It is most persuasive in the early part dealing with the basis of kingly authority. In the later sections, which describe the gradual popularization of political institutions, the narrative becomes less effective. Indeed, at times these chapters are essentially thumbnail histories of the countries under consideration, switching from theme to theme, looking randomly at political, economic, social, and cultural developments, always intelligent, often discursive, occasionally wrong. What we have here intermittently is not comparative but juxtapositional history, in which the decisive national experiences of several countries appear side by side without an integrative principle to give them coherence. Is it that the narrative loses some of its vigor in the second half, or is it—the thought began to trouble me as I continued to read—that I am simply more familiar with the recent period of history? Would someone better informed concerning the early period find weaknesses there as well? I cannot be sure, but I remain impressed by the bold purposes and sharp insights of the book. It offers an analysis of the process of political modernization that is original, perceptive, and generally convincing.

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WILLIAM H. MCNEILL and RUTH S. ADAMS, editors. *Human Migration: Patterns and Policies*. Bloomington: Indiana University Press, in association with the American Academy of Arts and Sciences. 1978. Pp. xviii, 442. \$22.50.

*Human Migration* is a treasure among the riches of the bicentennial harvest. Nineteen essays, first given as papers at a conference organized by Ruth A. Adams and William H. McNeill for the Midwest Council of the American Academy of Arts and Sciences, deal with "one of the most significant manifestations of the human condition" (p. xviii). Parochialisms are largely left behind, and McNeill's universal-historical orientation ties these studies, diverse as to topic, approach, and sensibility, into a meaningful whole.

The beautifully produced volume intends to stimulate scholarly dialogue across methodological and geographic frontiers and to lead, possibly, toward a revision of existing national or international policies. This twofold goal is to be reached, first, by a "sampling of historical patterns," second, by a globally oriented "survey of movements since World War Two," and third, by "a weighing of public policies" (p. xv).

In part one McNeill sketches some three thousand years of Eurasian migrations, mainly on the basis of epidemiological observations. James Lee features the migrations since about 1000 B.C. from which the "modern Chinese state and people evolved" (p. 39). The European scene since 1500 is reviewed by Charles Tilly, who stresses "the high mobility of the preindustrial world" (p. 67). Although one might quarrel with various generalizations, the three essays grant captivating glimpses of the significance of migrations in the Eurasian past; parallel sketches dealing with Africa, India, Southeast Asia, and the Americas are missed all the more.

The eight essays of parts two and three are devoted to recent "patterns of migration." P. A. Morrison and J. P. Wheeler reformulate Frederick Jackson Turner's frontier thesis into a migration thesis: "To the pioneering personality," widely dispersed over the United States, "almost any place will do so long as it is *another* place" (p. 82). H. J. Hoffmann-Nowotny understands the "guest" worker phenomenon of Western Europe as a typical example of *Unterschichtung*, the formation of "a new social stratum beneath the existing social structure of the immigration country" (p. 91). Orlando Patterson describes the complex system of migrations in the Caribbean and probes their psychosocial significance as expressed in archetypal symbols. Carl Solberg's chronological survey links Argentine mass migrations between 1870 and 1970 to international economic

structures. A. A. Bennigsen and S. E. Wimbush describe and interpret the state-directed and strategically perceived influx of some one and a half million Great Russians, Ukrainians, and Baltics into the four Central Asian republics and Kazakhstan. P. D. Curtin then sketches various types of migrations in sub-Saharan Africa, T. G. McGee the urban-rural mobility in south and southeast Asia, and Janet Abu-Lughod the "war-induced migrations" and those resulting from economic realities in the Arab world. The various essays often also address major theoretical questions and create an impressive mosaic of recent human migrations within sharply perceived historical contexts.

Part four, devoted to "problems of theory and policy" is, however, less satisfactory. It lacks clear focus and some of its eight essays are overly general and superficial. But Sune Åkerman's probing description of emigrational processes on the basis of Scandinavian data is incisive, and C. A. Auerbach's analysis of the freedom of movement as a human right in international law is illuminating. G. M. Rosberg provides a welcome, amply documented anatomy of the dimensions, causes, and effects of illegal immigration, especially from Mexico, to the United States; he rightfully warns against a "mood of hysteria" (p. 364), born of nativist-racist apprehensions, and he calls for an honest acknowledgement of this "de facto labor importation program" (p. 362) comparable to that of Europe.

*Human Migration* dramatizes the basic unity of internal migration within, emigration from, and immigration to an area. It highlights the impact of war, colonialism, national policy, and exploitative economic systems upon migratory movements. It provides a wealth of statistical and bibliographical data about lesser known regions and invites a critical re-examination of a host of theoretical assumption.

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CHARLES TILLY, editor. *Historical Studies of Changing Fertility*. (Quantitative Studies in History.) Princeton: Princeton University Press. 1978. Pp. ix, 390. Cloth \$27.50, paper \$10.95.

This is an excellent collection. The subjects are how fertility is structured, particularly by mortality, economic factors, and social constraints, and why fertility has varied over time. Three theoretical essays and five more empirical investigations treat important dimensions of these two topics.

In his introductory essay Charles Tilly integrates historical demography into central issues in economic and social history. He focuses on the demo-

graphic components and implications of the transformation of a predominantly peasant population into an urban-industrial one. Although he emphasizes the divergence between peasant and non-peasant demographic regimes, Tilly also argues for continuity in the theoretical approach to fertility in eras before and after the secular decline in the birth rate.

The economist Richard Easterlin synthesizes the demand side of the economic approach to fertility with the supply side formulated by sociologists. Easterlin's comprehensive scheme defines areas of relevance and irrelevance for each theoretical component. In the era in which couples have all the children possible (natural fertility), demand factors are unimportant, at least for individual behavior. This position conflicts with Tilly's emphasis on continuity in the theoretical approaches to fertility before and after the demographic transition. E. A. Wrigley also emphasizes the contrast between natural and controlled fertility regimes. Using a model relating the level of mortality to the probability of having an heir, Wrigley explores the possibility of unconscious rationality in premodern populations. Although families in early modern Europe may have overensured against the failure of male heirs, uncontrolled fertility was rational for the group in a no-growth situation. The distinction between the collective or unconscious rationality of premodern populations and the conscious rationality of individual couples in modern societies is also the philosophical difference between the basic assumptions of sociologists and economists.

The empirical papers in this volume also make significant contributions to the understanding of fertility in the past. Using econometric techniques on English and Swedish time-series data, Ronald Lee indicates the importance of exogenous variation in mortality in determining both population size and the level of real wages in the premodern era. He argues on both theoretical and empirical grounds that marital fertility varied over time in that period; conscious control is the suggestive implication of this result. Etienne van de Walle shows that low marital fertility in French departments before 1850 was strongly correlated with a higher level of nuptiality as well as with a higher expectation of life and greater wealth. He discusses literary evidence concerning contraception and cautiously explores alternative explanations for the departmental patterns. Lutz Berkner and Franklin Mendels use comparative examples and French departmental data for 1856 to examine the effects of inheritance on demographic behavior and household structure. Given the right circumstances, inheritance can be quite important: independent peasants in relatively uncommercialized areas who practiced impartible

inheritance had more complex household structures and a lower rate of population growth than peasants in regions of partible inheritance. Given the systemic character of demographic phenomena, one wishes that van de Walle and Berkner and Mendels had included the rate of natural increase in their analyses.

Rudolf Braun delineates the difference between the peasant demographic system and the system that emerged in the protoindustrial areas of the Swiss canton of Zurich in the eighteenth century. He suggests that opportunities in rural textile production not only promoted population growth through earlier marriage, but also fostered a more individualistic and romantic spirit. Braun's contribution illustrates the value of combining demographic history with the history of *mentalité*. Maris Vinovskis demonstrates that measures of economic development were the most important set of predictors in a multiple regression analysis of fertility ratios among Massachusetts towns in 1860.

These essays obviously are of major importance to scholars interested in demographic history. Social and economic historians, as well as those seeking illustrations of new methods in historical research, will also find this volume to be of considerable value.

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MICHEL FOUCAULT. *The History of Sexuality. Volume 1, An Introduction*. Translated by ROBERT HURLEY. New York: Pantheon Books. 1978. Pp. 168. \$8.95.

In one short and powerful volume the noted French historian and philosopher Michel Foucault has dramatically changed the field of sexual history. Since its rise to semirespectability in the 1960s, the intellectual framework that dominated the field was a simple one. In general, the West, under the influence of what Vern Bullough called a "sex negative" religion, rarely celebrated sexual license. Underneath this pall of disapproval, however, there were periods of tolerance such as the eighteenth century followed by sexual dark ages like the nineteenth century. In our time, because of Freud, changing economic patterns, family life, and religious beliefs, a revolutionary and promiscuous explosion that is popularly called the sexual revolution has taken place.

Foucault—as always unconventional and provocative—has the uncanny ability to stand aside from traditional interpretations and view historical process and the modern world with a fresh eye. At the end of this book he reflects on how a visitor from an age far in the future might view our intense pre-

occupation with sex and in a very real sense, Foucault himself is that visitor. What is unique about his vision is his insight that the cycle of license, repression, and revolution from the eighteenth century to the twentieth is far less significant than the development of sexuality as a major topic of discourse in modern times—as characteristic of the nineteenth century as our own time. For Foucault the growth and sustained interest in sex raises a whole new set of questions: most importantly, why has this occurred?

The answer to this question is complex and Foucault only begins to deal with it in the first volume (of six) in this series. Readers of Foucault's other books, in particular *Discipline and Punish*, will not be surprised to find that part of the answer is to be found in the nature of power relationships in the modern West. The emphasis on control, surveillance, confession, and discipline so brilliantly developed in the author's earlier accounts of asylums and prisons finds its counterpart in the enormous interest, for example, in the masturbating child who lives in a world of surveillance, discipline, and confession to compensate for his lack of control.

In this as in other volumes, Foucault's use of the word power is suggestive. In discussing sexual power, he is not speaking of the power of one class over another—the lower classes were relatively immune from middle-class sexual ethics—or the power of one group like the medical profession over women and children in the nineteenth century. These more obvious forms of power are less significant than the growth of what Foucault labels biopower, “an explosion of numerous and diverse techniques for achieving subjugation of bodies and the control of populations” (p. 140). This use of the word power allows Foucault to reshape the traditional framework of sexual history. For above and beyond the tightening and loosening of sexual attitudes and practices is the intense concern about sexuality. Being sex negative or sex positive seems less important than the centrality of sex itself and its role in the system of bio-power.

Although a longer review and analysis is in order, perhaps a brief statement of why this book is significant will be enough to whet the reader's appetite. If one takes Foucault seriously, the subject of sexuality can no longer be considered ancillary to women's history, family history, or other major interests of historians of modern social history. The conventional approach with its emphasis on repression and liberation, although of interest to some, could be relegated by most historians to the sidelines where it is worth a glance but not much more. With Foucault, sexuality moves to center stage because of the centrality of discourse about it from the eighteenth century to the present. It shares billing with

a wide variety of other mechanisms of surveillance and power in modern times and its study becomes an integral part of what Foucault in all his work is attempting to achieve: an insight, a definition, a description of what it means to be modern Western man.

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KENNETH W. WACHTER *et al.* *Statistical Studies of Historical Social Structure*. (Population and Social Structure: Advances in Historical Demography.) New York: Academic Press. 1978. Pp. xxiv, 229. \$17.50.

Historians have been actively interested in making their research more quantitative at least since the arrival of the computer. The problem that the authors of this book tackle is essentially how to relate the concerns of social historians to the techniques of computing. In between, concepts must be made clear, ideas based upon them, theories formed, models built, and data found that can test these models before computing techniques themselves can be applied.

Where one is only concerned with straightforward demography, the methods now in use are perhaps sufficiently well known, but Peter Laslett's main concern is with family and household structure, and this is a complex affair. The possible categories are surprisingly many, and in this volume some attempt is made to assess how far birth and death rates determine household size. The main conclusion, not surprisingly, is that social custom has a greater influence than simple demographic rates on household size. Simulations based on the population of Ealing as listed in 1599 and extended for one hundred fifty years according to various assumptions show this in a way that takes account of the relatively small size of that parish. (The reviewer feels a tinge of regret that they could not have made it three hundred fifty years; he was born in Ealing and was living there in 1949). They were able to study various systems of household decision rules through the simulations. Unfortunately, real communities that can be studied are usually rather small, and a wide range of possible systems would fit them. Repeated listings were very rarely made for the same community, and it is usually therefore only possible to determine the structural rules of a community within rather wide limits by this approach.

The last half of the book is not greatly connected with this rather lengthy simulation study. A brief analysis of the English baronets created by James I suggests that the extinction rate scarcely responded to variations in demographic factors, although the

measured rate against time shows a slight but disturbing tendency to increase, rather than to decrease as branching theory would predict. Historians may not have realized that no extraordinary factors need be invoked if half the families in a community are extinct within one hundred fifty years.

Elaborate analysis of some medieval economic data reveals the limitations of the statistical method when the author remarks, "There were only seven households with trees. . . . The same problem is raised by the effect of cows on taxes, when we know that all four [*sic*] cows in the sample were in one household. Some of these questions cannot be solved with the data at hand" (p. 150). The analytical method, in short, is too powerful for the data.

Another essay shows that everyone alive today of wholly English ancestry is probably descended from all the 86 percent or so of the population of 1200 or earlier who left any descendants at all. In fact, in a closed population, the proportion leaving any eventual descendants can only decrease the longer we look ahead, but very slowly. On the other hand, around 1600, his total ancestry would only have been around 0.2 percent of the total population then alive. The remaining two chapters are concerned with simulating the incidence of orphanhood with age under various assumptions and with the variability of age pyramids in village populations.

Emphasis is repeatedly laid on the smallness of the sample sizes used whenever data are not collected on a large-scale basis, such as a modern census. It is often tempting to generalize from no more than a few hundred cases, especially if they have been personally collected and analyzed. So much work seems to justify fairly firm conclusions, but simulation results show that they may be specious, which represents a challenge to social historians. Statistics may merely show that some questions cannot be answered.

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JACOB VINER. *Religious Thought and Economic Society: Four Chapters of an Unfinished Work*. Edited by JACQUES MELITZ and DONALD WINCH. Durham, N.C.: Duke University Press. 1978. Pp. 211. \$11.75.

This slim volume was published as a filial tribute to Jacob Viner by two of his distinguished students, one in England and the other in France. The four chapters were originally meant to be Viner's contribution to an ambitious project entitled "Religion in American Life," which was to be published by Princeton University Press. His assignment was to

write an introductory essay on the European background from the vantage point of an economist and intellectual historian. The work, however, became large enough to require a separate volume. Yet Viner was never to finish it. His first three chapters were deposited with the press in 1964 for safe-keeping, but the bibliographic references did not go beyond 1961. For the remaining six years of his life until his death in 1970 he continued working on the fourth chapter, which dealt with the Weber thesis and Protestant ethics, while at the same time completing and publishing numerous other works. Hence, this chapter was left even less complete than the earlier ones.

The first three chapters form a more or less continuous historical argument. In the first of these Viner deals with the economic doctrines of the Christian fathers, putting emphasis on their otherworldliness and their attitudes toward wealth, property, economic inequality, and alms giving. With some exceptions he finds among them neither much interest in worldly affairs nor any desire to reform the material world. In fact, one, Theodoretus, actually defended the existing economic society. In the following chapter, the author turns to the teaching of the scholastics on some of the same points he already covered in the previous chapter. But in addition to these topics he also examines some more typically medieval issues in the light of natural law principles, such as the doctrines of a "just price" and of usury. His treatment of usury is, within the limits of space, particularly thorough and fine, leading the ordinary reader with sure step through the normally incomprehensible tangle. He also points out that usury enjoyed an exceptional treatment at the hands of the scholastics inasmuch as they used more strictly nontheological reasoning for this subject than was customary. The third chapter takes us on to the secularizing tendencies of later Catholic thought, with special emphasis on the Jansenist-Jesuit controversy. In an enlightening exposition he makes the Jansenist Pierre Nicole an unintentional pioneer of nineteenth-century economic liberalism, albeit with some reservations. In the final chapter, as already indicated, Viner subjects Max Weber's thesis that the nature of "ascetic" Protestantism produced a special kind of capitalistic spirit to a searching and unfavorable examination and then appends a discursive account of how the idea of Protestant economic superiority was handled by writers before Weber.

Unfinished as these chapters are, they are sufficient to bear witness to the vast scholarship and erudition, as well as the intellectual breadth, of this unusual economist, one of an almost extinct species. There are valid criticisms of his work, most of which have already been anticipated by the editors in



their introduction. The obvious one is that the bibliography needs to be updated. A more cogent criticism is that Viner was not nurtured on patristic and scholastic literature and consequently had to tread cautiously and sometimes too selectively. This tendency is especially glaring in the second chapter, where in places he relies almost exclusively on Thomas Aquinas, whose influence by his own admission only became prominent in the sixteenth century. Nevertheless, the book is a worthy memorial to a humane scholar and a challenge to take up his torch, perhaps by collating religious thought with the latest findings in socioeconomic history.

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OTTO DANN, editor. *Nationalismus und sozialer Wandel*. (Historische Perspektiven, number 11.) Hamburg: Hoffmann and Campe. 1978. Pp. 240. DM 36.

The six studies in this volume are based upon the theories of Karl W. Deutsch (see his *Nationalism and Social Communication: An Inquiry into the Foundations of Nationality* [1953]). Deutsch believed that a social science of nationalism might be developed, which would enable us to predict and control events and permit us to prevent nationalistic conflicts from leading to national and social destruction. None of the authors here seems to assume that their investigations are laying the groundwork for eventual world unity, as Deutsch suggested such studies could and would. They do adopt Deutsch's theories and his definitions. Each essay presents material that conforms to Deutsch's view that the origins of national consciousness in a people are dependent upon the expansion, intensification, and alterations in communications. This integration is an early stage in the modernization theory of political development, the usefulness and validity of which the authors presume, which constructs a basic model of developmental problems that every society in the process of modernization must overcome. "Nation-building," one stage in the process, solves the problem of "identity and legitimacy."

The essays concern themselves only with nationalism in its earliest phase, when it appears as a freedom movement, "liberal in all its features." Extreme forms are said to appear mainly in established national states and are not examined. The essays are "Aristocracy and the National Movement in Poland," by Kurt Hausmann; "Nationalism and Social Change in Germany, 1806-50," by Otto Dann; "Nationalism as an Agrarian Mass Movement in Ireland, 1879-86," by Peter Alter; "The Bourgeoisie and the Risorgimento," by

Hartmut Ullrich; "Regionalism and Social Change: The Catalan Example," by Gerhard Brunn; and "Nationalism and Social Change in the Third World: Twelve Theses," by Dietmar Rothermund. Each author deals with the "nation-building" stage, during which classes and regions that were previously separated develop common needs and goals and the function of which is political democratization, because the goal of national movements is "the participation of the nation in political decision making."

Dealing with nationalism of different epochs, societies, and regions—in relation to modernization and social change—each author seeks to demonstrate that nationalism is always the result of previous change coupled with the modernization of traditional classes. The authors conclude that nationalism is not a bourgeois ideology in the Marxist sense. The essays on Italy and Germany demonstrate, if demonstration were needed, that neither the modern bourgeoisie nor specific economic interests dominated the national movement in these countries.

Scholarship undertaken to demonstrate the usefulness of a theory or of a social scientific model of national development is limited in its significance and interest by the soundness and originality of the theory and by the degree to which the model conforms to the full complexity of historical experience. One's judgment of Deutsch's work will largely determine one's opinion of this collection.

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J. R. V. PRESCOTT. *Boundaries and Frontiers*. Totowa, N. J.: Rowman and Littlefield. 1978. Pp. 210. \$20.00.

What are the primary factors that determine the position and character of boundaries and frontiers? What part do geographical features play in establishing boundaries and frontiers? How do these demarcations affect the development and policies of the separated states? These questions, which were asked in the author's 1965 book entitled *The Geography of Frontiers and Boundaries*, are re-evaluated in this new, completely revised volume.

To set the stage, J. R. V. Prescott reviews and summarizes the main concepts of nine major writers, such as Ratzel, Curzon, and Boggs, whose work spans the period 1897-1945. This is an important aspect of the book, because the ideas of these writers have provided the foundations for subsequent scholars. The author concludes the chapter by defining ten key terms used in the book that re-

late to boundaries, designated as lines, and frontiers, defined as zones. The second chapter then examines the characteristics of primary frontiers, which exist when a state is taking possession of its territory for the first time, secondary frontiers, which mark zones that separate settled and uninhabited regions of a state, and political frontiers, which used to separate neighboring countries. The final aspect of this chapter deals with an account of frontiers in Nigeria.

The next chapter describes and explains the evolution of boundaries in definition (that is, by allocation, delimitation, and demarcation), in position, and in function. The author concludes this chapter with a summary and discussion of the evolution of the boundary between Mexico and the United States since 1847. The author carries the flow of this latter topic into the next chapter by examining boundary disputes. As in the previous chapters, the author methodically points out that the general term "boundary dispute" includes four quite different types of disagreements between countries. A discussion then follows with examples of territorial, positional, and functional boundary disputes and a last type of dispute that concerns the use of trans-boundary resources such as a river or a coalfield. A considerable amount of updated material from the previous book is used in this chapter.

Chapter five deals with maritime boundaries. The material is completely new and rather timely because of the recent concern by coastal countries for the peaceful, orderly exploitation and conservation of the ocean's resources. Prescott discusses the origin of national maritime claims, the six maritime zones, which are or will be subject to national or international authority, the operational definitions for baselines, the edge of the continental margins, and the division of maritime zones between adjacent and opposite countries. Despite the fact that agreements have been made, for example, on maritime zones, some countries face internal problems related to coastal water claims. In Canada, provincial versus federal jurisdiction of waters is a question that is constantly raised by coastal provinces.

Chapter six considers the two main types of intra-national boundaries—federal and internal. Prescott explains that federal boundaries separate states within a federation, while internal boundaries mark the limits of administrative units within the federal states or unitary states. In total, the reader should view this classification as a hierarchy of boundaries—international, federal, and internal—with the last having a division between local government boundaries and field service boundaries.

The final chapter explores the nature of border landscapes associated with international, federal, and local boundaries. Within this area of study Pre-

scott discusses the contributions that geographers can make in understanding the influence of political factors upon the cultural landscape.

In summary, the author has produced a concise, systematic introduction to the characteristics and functions of boundaries and frontiers. The entire volume is documented with both examples and maps. On this latter aspect, not only has the author provided maps for the reader to understand the evolution of boundaries and frontiers, but he has also indicated the use of maps in understanding the nature of the basic problems that arise. Not only will this volume be of interest to political geographers, it will also be of use to political scientists and political historians.

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ANDRE GUNDER FRANK. *World Accumulation, 1492-1789*. New York: Monthly Review Press. 1978. Pp. 303. \$16.50.

In this grand effort of historical synthesis, Andre Gunder Frank attempts to locate the origins of what he has previously termed the "development of underdevelopment" in the context of the rise of the international capitalist system. The author's goal is to explain the variable impact of the accumulation of capital in Western Europe on capital accumulation and the development of internal economic relations in different colonial and geographical regions during the period 1492-1789. The various colonies of Asia, Africa, and the Americas receive attention at those points in time when, in Frank's opinion, they played a key role in the process of world capital accumulation.

Historians of any particular time and place that Frank covers (or rather touches upon) will undoubtedly find the analysis of their specialty much too condensed and oversimplified, even if they happen to agree with the author's general line of argument; and they may also find, as did this reviewer in a few places, that the author has failed to consult important secondary works that would either have greatly strengthened or significantly modified particular conclusions that he draws. Clearly, such problems are inherent in this type of historical sociology, the main virtue of which, if done adequately, is to focus our attention on the larger theoretical issues of social development and change at the risk of empirical omissions and oversimplification. Indeed, Frank goes to great lengths in his introduction to the book to warn that, however well founded and self-confident his analysis may appear, it is certainly "full of holes" that will require further empirical research and theoretical reinterpretation if they are to be



filled. In this context of intellectual dialogue, the purpose of the book is to help to ensure that the "holes" that social analysts (including the author himself) attempt to fill in the future will be the most relevant ones for comprehending the historical relation between the rise of world capitalism in the sixteenth through eighteenth centuries and the modern international division of labor.

From this perspective, the book is clearly useful, particularly to those readers who already share Frank's Marxist orientation toward analyzing capitalist development and who can hence most readily relate its contents to a much broader literature and to more general debates as well as to their own developing view of the world. It will also be helpful, as a form of textbook, to students who know little about the rise of capitalism; it reads well, pulls together a wide range of secondary literature, and the analytical approach should stimulate further reading. But, on the whole, the book lacks the analytical rigor that could have made it an important contribution to the analysis of social change and that might even have reoriented the thinking of many readers not previously disposed to the Marxist perspective. Perhaps a prime reason for the lack of analytical rigor is Frank's annoying propensity to quote "authorities" at length in the body of the text as a way of making his own arguments, thus evading the necessity of carefully assessing the logical consistency of the empirical evidence and developing a concise and rigorous argument in his own words. Such a practice is unnecessary since all the works he cites are easily accessible, the result being simply to substitute the holes in someone else's argument (which, in addition, may be based on a different analytical framework) for the holes that Frank might have had to reckon with by a more careful development of his analysis.

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NORMAN JACOBSON. *Pride and Solace: The Functions and Limits of Political Theory*. Berkeley and Los Angeles: University of California Press. 1978. Pp. xv, 166. \$10.00.

Although this essay by Norman Jacobson devotes three of its five chapters to historical figures, its focus is on the scope and limits of political theory in the middle of the twentieth century. Machiavelli, Hobbes, and Rousseau are discussed, often very instructively, as representatives of an older tradition of political thinking, now defunct, that tried to provide solace in the face of political disorder through developing general, rational principles of political association and legitimation. Jacobson is very good

at showing how the prescription advanced for the sake of the commonweal was also to function as therapy for the disordered psyches of men experiencing in themselves personal versions of the public conflict over the right proportions of liberty and repression, nature and society.

The remedies offered by the three traditional political philosophers presumed the power of reason to understand, harmonize, and quiet disruptive passion. In Jacobson's view, no rational man in this modern age believes that reason can establish saving general principles, and this skepticism is one important factor in determining the scope of modern political theory. Another decisive factor has to do with the power of the state. In moving from Machiavelli to Hobbes and on to Rousseau, the author notes that the solace offered by these thinkers involved ever greater degrees of state authority over the beliefs and actions of citizens. This call for increased state power was accompanied, most obviously in the case of Rousseau, with progressively more radical revelations of the unjust and corrupt basis of all claims to political domination. In Jacobson's view, what has become clear in the twentieth century is that the human price of all statist solutions is morally intolerable. The state stands exposed as amoral and reason proves itself impotent.

Thus, humane political theory in our age can bring us solace only as it prevents the triumph of fanatics who claim to know all, and it can do that through establishing a set of limits for political action. Political theory becomes the theory of prevention, passionately determined to defend moderation as an ultimate moral necessity. The pride of modern political thinkers is expressed not in discovering universal principles but simply in refusing to submit to absurdity. Jacobson's heroes—George Orwell, Hannah Arendt, and Albert Camus—are people of limited hopes and great moral passion.

It is Camus's thought that especially appeals to Jacobson, and he expounds it as an advocate. He insists on the wisdom of Camus's belief that in the face of unescapable absurdity and ambiguity we must not avoid the moral responsibility of becoming engaged with the world. But our expectations and our understanding must be sharply limited if we are not to become immoral fanatics in our turn. This pessimism seems to derive not simply from a consideration of the suffering brought about by twentieth-century ideology but from the conviction that any claim to rational insight and understanding must lead to such moral disasters. Any claim to "truth" is dangerous because it easily leads to murder, a point Jacobson makes several times as he quotes and paraphrases Camus. Hence, only as we are aware of our ignorance can we behave humanely.

Jacobson never makes it clear why the claims of ideological fanatics to rational knowledge of basic principles must make reason itself suspect. Is it not conceivable that the horrors brought upon the world by Marxists and fascists have something to do with the radical irrationality of these secular religions? Jacobson's bleak views have much to do with an implicit theory of the nature of reason and its relation to moral action, a theory that is neither defined nor defended here because it is presumed to be obvious. Far from being obvious, it seems highly dogmatic and superficial. Humane as Jacobson's concern is, the edge of near despair evident in the pages of this book seems to be purchased too easily. A therapeutic discussion of the pathology of modern politics and political morality would have to inquire much more radically into fundamental assumptions about the nature of reason itself. What we have here is more a symptom of the problem than a prescription for its cure.

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JULES STEINBERG. *Locke, Rousseau, and the Idea of Consent: An Inquiry into the Liberal-Democratic Theory of Political Obligation*. (Contributions in Political Science, number 6.) Westport, Conn.: Greenwood Press. 1978. Pp. 155. \$14.95.

Jules Steinberg challenges the common liberal belief that governments derive their legitimacy from the consent of the governed. His book provides a rigorous analysis of the ways liberal-democratic theorists have used the idea of consent as the basis for a theory of political obligation. Primarily a contribution to political theory (the book appears in Greenwood's "Contributions in Political Science" series), the study will also interest historians because of the importance of the topic and because the argument is in part historical. Steinberg seeks to demonstrate that consent theory is logically incompatible with the commitment to moral diversity and pluralism that has generally characterized liberal thought since Locke. He argues, moreover, that the idea of consent is unnecessary and should be abandoned, because this same tradition embodies a perfectly adequate alternative theory of political obligation.

The idea of consent lives on, Steinberg contends, because it appears to moralize obedience to law. By a careful logical examination of the reasoning that consent theorists use to link moral obligations to legal obligations, he shows that consent theory implies and uses a commitment model of moral obligation. In this formulation the individual is obligated to obey the law only because he has freely committed himself to do so. In contrast, Steinberg

points to teleological theories of moral obligation that appeal to conceptions of morally desirable consequences as the means of deciding what acts an individual ought or ought not to perform. Here the individual is morally obligated to obey the law, because he understands that this is what he ought to do, that obedience to the law is rationally correct. In this perspective the legitimacy of a particular government depends not on the consent of the governed but on the government's performance with respect to the objectives a government ought to promote.

Historically, Steinberg argues, John Locke used the idea of consent as a means of discussing the (limited) moral and rational purposes of government. In Locke's essentially teleological theory, individuals have a moral obligation to obey the law insofar as a government does what it ought to do, that is, to preserve the natural rights of individuals to life, liberty, and property. Steinberg believes that the logic of Locke's use of the metaphor of contract derives from the fact that individuals enter contracts in order to realize specific purposes rather than from the fact that contracts are devices by which individuals freely bind themselves.

In contrast, Jean-Jacques Rousseau provided a coherent consent theory of political obligation. But, as Steinberg demonstrates, the logic of Rousseau's position involved an explicit rejection of what Locke had assumed—the legitimacy of selfishly motivated conduct and the existence of a morally diverse and pluralistic society.

This is a solid study that adds to our understanding of Locke, Rousseau, and the issue of political obligation in a pluralistic society. The presentation is clear and convincing, if a bit repetitious. Every nuance of the argument is buttressed by generous footnote references to historical and contemporary positions. The documentation, however, is limited to English-language sources.

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G. L. ULMEN. *The Science of Society: Toward an Understanding of the Life and Work of Karl August Wittfogel*. New York: Mouton; distributed by Walter de Gruyter, New York. 1978. Pp. xxviii, 747. DM 175.

Exhaustively interpreting history the while, Karl August Wittfogel collided with a great deal of it in the Germany of the Weimar Republic, pre-Communist China, and the United States of the Cold War. He joined the Communist Party in 1920 and the Frankfurt School in 1924, debated with György Lukács and Karl Korsch, wrote Marxian plays and cultural criticism, and developed his ideas further as

an engaged Sinologist. In this country he joined but later turned against the Institute of Pacific Relations, which had been accused of harboring Communist agents. Wittfogel quit the Communist Party, but not Marxism, and became a severely judgmental—and severely judged—cold warrior.

Wittfogel's ultimate statement was his book, *Oriental Despotism: A Comparative Study of Total Power* (1957). The subtitle suggests the magnitude of his claim to comprehend "big patterns of societal structure and change" in a new science of society (preface to the 1962 edition, p. iii). He believed he had transcended Marx and Weber. In fact, Wittfogel had fitted together and hugely enlarged upon a casual and ignorant aside of Marx about the "Asiatic mode of production" and a more serious, but carefully limited, suggestion of Weber about the bureaucracy of irrigation works. Chiding Marx for having regressed from his insight when writing *Capital* and Weber for resisting Marxism, Wittfogel tried to prove that "large-scale and government-managed works of irrigation and flood control (hydraulic agriculture)" had created what he saw as the characteristic despotism of the Orient (p. 3). But, virtually ignoring India and other Asian countries, he arbitrarily took China as the ideal type of Oriental state and, arranging empty abstractions, provided no data on the location, size, management, and relation to government and society of the hydraulic works. Even if his thesis were sufficiently "correct" to remain standing, it would be overwhelmed to near invisibility by the grand architectonics of Marx and Weber. The book, met with a mixture of baffled praise and disparagement, was almost immediately abandoned by serious scholarship.

G. L. Ulmen, a former student of his, has compiled a massive catalogue raisonné of Wittfogel's life and works. At times, Ulmen might be documenting the progress of Buddha: "After short stopovers in Germany with relatives and friends, Wittfogel ended his global wandering with a lecture on hydraulic society at St. Antony's College, Oxford. It was early summer when he returned to New York" (*The Science of Society*, p. 458). More amanuensis than scholar, Ulmen has nowhere questioned a statement or action of Wittfogel's. He has served his teacher badly. Permitting Wittfogel to rule the book despotically, Ulmen has rationalized every inconsistency and denied every error while conceding scant credit to any competitive intellect.

Wittfogel deserves better: a sympathetic but critical study that would evaluate his achievements credibly and, if you please, more briefly. As he proceeded in his various wrong directions, he saw important truths out of the corner of his eye. His exploration of the ethos of Asia, despite his

Eurocentrism, and hydraulics, despite his extravagances, has encouraged fruitful scholarly inquiry. Rigid about his own ideas, he tortured and stretched Marxism to the point where it could fit loosely over independent thought.

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RICHARD SLOBODIN. *W. H. R. Rivers*. (Leaders of Modern Anthropology Series.) New York: Columbia University Press. 1978. Pp. xv, 295. \$20.00.

Claude Lévi-Strauss has described W. H. R. Rivers as the Galileo of ethnography (to Mauss's Newton), and perhaps Richard Slobodin is a little too inclined to accept such an extravagant evaluation. I share his admiration and secondhand affection for Rivers, but I would be more comfortable with the immediate concession that he is distinguished not for any massive discovery or original theoretical formulation as much as for the breadth of competence, the sensitivity, and the intellectual honesty he displayed in his work in psychiatry, neurology, psychology, ethnography, and social anthropology. It is peculiarly difficult to write interestingly about high competence, as opposed to genius, and the difficulty is compounded when the competence is so broadly displayed. By presenting Rivers perhaps too simply as a great scientist, Slobodin's book is ultimately less successful than it might have been.

This is not to deny that the bench marks of Rivers's career, set out so clearly here, are by no means negligible, even at this distance in time. In 1898 he participated in the Cambridge expedition to the Torres Straits, the first organized British ethnographic expedition, and there he conducted a path-breaking study of perception, demonstrating that perception is culturally influenced. In 1901–02 he made a study of the Todas, producing in 1906 a fine ethnography, and he followed this up with a survey of Melanesian societies. During these expeditions he invented and developed the genealogical method of inquiry. Beginning in 1903, he and Head carried out experiments on cutaneous sensation that were to influence a generation of neurologists. In the meantime he trained a small but talented cohort of Cambridge psychologists and anthropologists.

During the First World War, Rivers won a new reputation as chief doctor in a hospital that pioneered the treatment of war neuroses, an activity beguilingly described in the memoirs of one of his patients, Siegfried Sassoon. After the war, this protean man decided to enter politics and was adopted as Labour candidate for the London University

seat. Unfortunately, he died shortly before the campaign and was rather incompetently replaced by H. G. Wells. During these last years of his life Rivers had increasingly turned back to anthropology, which he decided was his true *métier*, only to be seduced by the meretricious Egyptocentric diffusionism of his friend Elliot Smith, a development that gravely detracted from his reputation in later years—not altogether unreasonably.

Richard Slobodin provides an unpretentious and reliable account of Rivers's career, if failing to contextualize it properly, and a solid and professional evaluation of his main contributions, particularly to anthropology. As the eighty-page selection from Rivers's work included here bears witness, however, his hero is in no sense a contemporary figure (Slobodin to the contrary), and a more strictly historical attempt to grasp what Rivers was about in his time and place would have been preferable. Here Slobodin is less strong, but this is a valuable source on an unusually attractive and diverse scholar.

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#### ANCIENT

K. J. DOVER. *Greek Homosexuality*. Cambridge: Harvard University Press. 1978. Pp. x, 244. \$22.50.

K. J. Dover is well known among scholars of Greek antiquity for his painstaking and controversial assessment of ancient Greek (especially Athenian) mores. In *Aristophanic Comedy* (1972), he argued convincingly that the plays of Aristophanes show exactly what the living language of fifth and fourth century Athens expressed as humorous, as distinguished from obscene, and why Athenians laughed heartily at the myriad of puns, jokes, allusions, and open references to sex, scatology, homosexuality (both male and female), and aphrodisiacs. He also has indicated the variety of sources available in his *Greek Popular Morality in the Time of Plato and Aristotle* (1974), which foreshadowed *Greek Homosexuality*. To the disgruntlement of many classical scholars, Dover has emphasized Attic oratory rather than philosophy to gain a view of common assumptions, and pages 213–16 of *Popular Morality* (“Sexual Behaviour: Homosexuality”) cast themes that are expanded in *Greek Homosexuality*. Several passages in *Popular Morality* admirably summarize Dover's basic conclusions on Attic homosexuality and will aid considerably in the comprehension of the full monograph on the subject: “Homosexual relations provided a youth, for whom marriage lay some years ahead, with the opportunity for the seduction of a partner on the same social plane as himself, an

opportunity of the kind which exists in modern heterosexual societies which neither own slaves nor segregate the sexes. At least from the sixth century onwards, the Greeks regarded homosexual desire by a man or youth for a boy, or by a man for a youth, as natural” (p. 213); “homosexual courting and importuning is a very common subject in vase-painting . . .” (p. 214); “If a man was proved to have prostituted himself for money, as a boy or at any subsequent time in his life, he lost citizen-rights in perpetuity, to the same degree as a man who was unable to pay off a financial debt to the state” (p. 215); and “Discussion of Greek homosexuality in works of the last hundred years, being too much influenced by modern European law, has in general failed to see that Greek laws did not penalize an unnatural act *per se* but were concerned with the civic status of the participants and with the relationships of which the act was an ingredient” (p. 216, n. 23). *Greek Homosexuality* sets out the full range of evidence to back these conclusions, curiously unstated even in the concluding chapter, “Myth and History.”

Beginning with the questions of evidence, Dover delineates why philosophy is a poor guide to ordinary morality. Attic orators are more reliable, but they too must be treated with caution, because they will state opinions that listeners presumably wanted to hear. Vocabulary will loom as important; sure command of the euphemisms, nuances, and contexts is essential. All traditional sources receive far better understanding when Dover includes—for the first time on a full scale—the artistic evidence seen in Greek vase paintings. He warns, however, that many scenes and “gestures are ‘culture-bound,’ and it is possible to make bad mistakes in interpreting them” (p. 5).

The famous case of Timarchus forms the core of Dover's arguments from Attic law. Within the subtleties of defense and legal attack, it is clear that Athenians took for granted prostitution and homosexuality but that they were very much offended if sexual favors were offered in exchange for payment and especially if a citizen became a professional  *pornos*. Thus there were social prohibitions among the Athenians concerning sex, and the legal evidence shows that disgrace came from flouting one's role as citizen by taking on the manners of a non-citizen, who was perfectly free to sell his or her body for sex. From law, Dover proceeds to what he calls “publicity,” and the comedies of Aristophanes illustrate how widespread this commonly assumed homosexual ethos was. In “Predilections and Fantasies,” Dover skillfully combines the evidence from comedy, poetry, and the vase paintings; Athenians had their sexual fantasies, just as is true of all societies. Penis size, jokes about circumcision, the abstract “body beautiful” of the ideal male, and similar stereotypes characterize the evidence, and “it may be legitimate



(or it may not) to detect the artist's preoccupation with the penis at work in the configuration of scenes which have no overt sexual content" (p. 133). Moreover, both Plato and Aristotle "exploited" a common assumption, and the *Problemata*, among several tracts, gives arguments that "a genital response to the bodily beauty of the younger male was [not] regarded as a defect . . ." (p. 170). Of signal interest is "Women and Homosexuality" (pp. 171–84), which suggests that women, as well as men, sought "all such genital acts as the inventive pursuit of a piquant variety of pleasure can devise . . ." (p. 183).

Far from being an *apologia* for homosexuality, *Greek Homosexuality* is a clear, nearly clinical assembly of the evidence pertaining to attitudes and practice of male and female homosexuality in ancient Athens. The one hundred and six illustrations, plus the beautiful color frontispiece of Ganymede by the Berlin Painter, will arrest even the casual browser with their simplicity, precision in execution, and a general air of being "ordinary." The vase paintings firmly indicate that erotic drawings and paintings were certainly not offensive, and these magnificent works of art formed part of accepted decor in the most genteel households. Those same vase paintings also depict heterosexual activity with a similar nonchalance, and the summary of evidence—Attic comedy, Attic orators, love poems ranging from Sappho to Hellenistic epigrams contained in the *Greek Anthology*, the philosophers and their reaction to sexuality, and the vase paintings—shows a culture with vastly different sexual mores than characteristic of the West in the twentieth century. Sex, however, was regulated: regulation came in the form of careful scrutiny of citizen status, which, in turn, linked with how the *polis* regarded its own religious underpinnings. Tainted bloodlines were an offense to the gods of the *polis*, and the legal evidence from Athens shows how deeply rooted were these notions concerning sexual (either homosexual or heterosexual) offenses.

*Greek Homosexuality* provides—finally—an unvarnished look at Athenian homosexuality for what it was. It is a carefully constructed book and is now the standard volume on the subject. Dover's command of the sources is, as always, full and sure, but the book suffers from an irritating hesitancy: the author never quite gives us what he thinks about all the materials at hand. Professors and teachers of all levels should have no doubts that this is the book to recommend to students and general readers. It has "the facts" as can be known.

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A. ARTHUR SCHILLER. *Roman Law: Mechanisms of Development*. Paris: Mouton Publishers; distributed by

Walter de Gruyter, Berlin. 1978. Pp. xxxvii, 606. DM 158.

This book is the result of years of preparation and experimentation in the classroom; and its original mimeographed format has allowed constant improvement. Although A. Arthur Schiller refers to it as a textbook in Roman law, it is not directed to undergraduate courses, which generally emphasize the content of Roman law as expounded in the *Institutes* of Gaius or Justinian. This book is, rather, directed to an advanced course on the sources and development of the Roman law of the classical period (approximately the second century B.C. to the third century A.D.).

The first third of the book treats the earliest periods of Roman history and presents background materials necessary for the pursuit of its main purposes. There are discussions of medieval and modern courses in Roman law, their source materials, and the status and fruits of research. The section on textual criticism (pp. 60–83) is the clearest exposition of this laborious field that I ever have seen. There also is an adequate discussion of preclassical law: its nature, its development, and the magistrates that administered and influenced it. The rigidities and difficulties of this period are illustrated by a discussion of the *legis actiones*.

The main body of the work, however, treats the sources of classical law: statute, custom, the jurists, the magistrates, the Senate, and the emperors. The work concludes with a consideration of the practical and the theoretical in classical law, both in Italy and in the provinces, with especially illuminating sections on the meaning and relations of such sometimes controversial words and phrases as *jus civile*, *jus gentium*, *jus honorarium*, *jus novum*, *aequitas*, and *jus naturale*.

Schiller's method of presentation is novel and very satisfactory. As each subject or problem is broached, he presents, after a few introductory remarks, all of the important ancient texts in English translation. These are followed by a discussion of their interpretation and an unbiased review of the controversies that they have generated. Repeatedly, he allows the student to make up his own mind, giving directions and a bibliography for further reading in depth on each subject.

Even Homer nods: in a book of this size and complexity there are bound to be misprints. They are not many, and they generally are inverted or omitted letters, with an occasional omission of a word. These usually required only a quick second reading to obtain the sense; but they are grouped in such manner as to warn us not to read proof when weary. There is one unexplained contradiction: two different statements as to an emendation of Pomponius by Mommsen (pp. 143, 146).



An appendix with the important dates in the history of Roman law is useful for keeping one's bearings; and there are indexes of the sources translated and of the subjects treated. This will be a required guide for advanced students of Roman law. The very extensive bibliography will lighten the tasks of students and investigators; but the frequent referral to the content of these works means that its most effective use as a textbook must be made where there is a considerable law library. Its usefulness is not, however, confined to advanced work. It will also be a welcome addition to the tools of the teacher of undergraduate courses in Roman law, for it will help in answering the questions of "how" and "why" that the better students of the *Institutes* invariably ask.

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### MEDIEVAL

EDWARD PETERS. *The Magician, the Witch, and the Law*. Philadelphia: University of Pennsylvania Press. 1978. Pp. xviii, 218. \$15.95.

Edward Peters, professor of medieval history at the University of Pennsylvania and editor of the Middle Ages series for that university's press, is well qualified to deal with the historical definitions of the magician and their connection with the notion of the witch. His use of the major kinds of sources of the medieval period (monastic, courtly, penitential, and theological) is quite sensitive and informative.

His organization is largely chronological, starting with pre-Christian sources. Although there are other works, such as Julio Caro Baroja's *The World of the Witches* (1964) and even Kurt Seligmann's *Magic, Supernaturalism, and Religion* (1971), which give some of the same material, no one has really singled out, as Peters has, the magician in this early period. He traces the concept through the literature to observe carefully the changes in its interpretation which were taking place by the High Middle Ages. He also examines the environment in which these comments were being made in order to explain why certain trends evolved. In this context, he is most successful in distinguishing between the comments directed to monks and those aimed at the princely courts.

It is Peters's contention that magicians and heretics had long been linked together in medieval literature and that it is the gradually emerging views about magicians and their actual prosecution in the fourteenth century that laid the groundwork for the prosecutions of witches in the fifteenth century.

Also important to his thesis is his assertion that, until the thirteenth century, the early sources for the growing objections to magic were literary and not theological or legal. Although the development of the Inquisition provided a legal vehicle for the prosecution of witches by the fifteenth century, far more important in that development were the invectives that were directed against magic and its practitioners.

Although his case is convincingly proved by his citations of many texts, in the opinion of this reviewer it reveals only part of the picture for two reasons. In the first place, throughout the book there is an underlying assumption that the citation of certain texts in chronological order according to their category will reveal intellectual development and, thus, the interaction of some texts or writers with others. In many instances, therefore, Peters assumes without totally convincing proof that, because one author has ideas similar to those of a predecessor, a natural progression of thought from one to the other had taken place. In this context, he frequently avoids dealing in any depth with the interaction between popular opinion and literate ideas.

In the second place, Peters does not include much of the material that has recently been published on the development of Christian demonology, the increase of heresy, and the intellectual renaissance of the High Middle Ages; thus, his own assertions sometimes seem one-sided and out of context. That omission, however, is less true in some of his later chapters where he explores the theories of several secondary writers in the light of his own findings. For example, his use of Peter Brown's theories about the late Roman Empire as they apply to the fourteenth century gives real dimension to his own thesis.

Peters's book includes three appendixes—a very interesting one on the use of torture and what that implies about the society using it, another essay on Nicholas Eymeric's *Directorium inquisitorum*, and a third one on the most recent books about the history of the magician and the witch. Despite many printing errors, this book is an important contribution.

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UTA-RENAME BLUMENTHAL. *The Early Councils of Pope Paschal II, 1100–1110*. (Studies and Texts, number 43.) Toronto: Pontifical Institute of Mediaeval Studies. 1978. Pp. xiii, 173.

Pope Paschal II (1099–1118) is commonly characterized as an inflexible, monkish adherent of the

Gregorian reform program, a man whose rigidity renewed and prolonged the Investiture Controversy with the Empire. Paradoxically, he is also generally discounted as an unrealistic politician who was prepared to concede too much, chiefly because in 1111 he tried to resolve the investiture problem by depriving bishops of their temporalities, and, failing that, by authorizing the emperor to practice lay investiture. Although neither of these radical solutions proved feasible, he did effect a permanent and realistic compromise in England by letting the bishops there do homage to the king for their temporalities. Obviously, Paschal was a paradoxical pope, whose character and policy are open to the most diverse interpretations.

In the past it has been all the more difficult to form a balanced estimate of this pontificate because the sources are scanty and scattered, but the early conciliar ones have now been put in order by Uta-Renate Blumenthal. When her study was submitted as a doctoral dissertation at Columbia in 1973, its character was made explicit by the subtitle, "A Text-Critical Study." It is a credit to her mentor, Robert Somerville, and to his method of editing and elucidating the history of councils. She treats seven church councils in chronological order, and discusses each systematically under four headings: sources and previous scholarship, participants, canons, and other notable decisions. This work is a model of its kind, with an impressive and probably exhaustive array of documentation. Unfortunately, no one scholar can search every manuscript collection in search of the fugitive fragments of conciliar legislation. Ideally, a collaborative international survey of the manuscript sources should precede studies such as this; but, working chiefly in France and Italy, Blumenthal has been able to turn up new texts for four councils. To make a manageable dissertation topic, the scope of her study was wisely limited to Paschal's early councils; yet one wishes she had done the later ones as well before publishing. Given these limitations, what has been done has been well done.

What emerges from this technical source study is a clearer picture of the development of a legalistic mentality in the papal curia. Paschal was more attentive to precedents than were the earlier reformers, and he widened the range to include not only legal but also patristic and biblical authorities. Like a good lawyer, however, Paschal knew the loopholes; and, accordingly, he granted dispensations when it was politically expedient to do so. Blumenthal believes that he used his councils to discuss major policy decisions, but I think it more likely that they were occasions on which decisions that had been reached privately were solemnly made public. Students of the period will find many more

suggestive details, as well as a reliable set of texts accompanied by an invaluable wealth of documentation.

RICHARD KAY  
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RICHARD BARBER. *Edward, Prince of Wales and Aquitaine: A Biography of the Black Prince*. New York: Charles Scribner's Sons. 1978. Pp. 298. \$17.50.

Richard Barber describes his book as a biography of the Black Prince. Most of his readers will find this a somewhat misleading claim; owing to the limited nature of the sources, it is almost impossible to write a biography of any medieval figure, in the usual meaning of the word, that is a well-rounded study of a man's life, actions, personality, and intimate thoughts. The most an author can achieve for the generality of medieval figures is an outline of their external actions. Inner thoughts and motives have to be deduced from events, and this is a highly fallible hit-and-miss process. Barber's account of the Black Prince is no exception. It is, however, an extremely detailed and successful account of the prince's military career from the time when, at the age of fifteen or sixteen, he achieved a reputation for personal valor in the Crécy-Calais campaign of 1345-46 to the final, abortive Spanish expedition of 1367. More particularly, Barber attempts to test Froissart's famous legend of the prince against the more sober accounts of chronicles and official sources.

The narrative of the prince's campaigns is as sound as can be expected from the nature of the sources, which, as the author readily admits, leave a fair amount of room for hypothesis and speculation on strategy, tactics, and the details of particular battles. It is impossible, for example, to determine with any certainty Edward III's strategy before Crécy. Very occasionally there are unfortunate lapses. At one point the author refers to "huge English and French armies" (p. 26). Such a statement could seriously mislead students. The armies of the Hundred Years War, at least on the English side, rarely exceeded ten thousand men and were often much smaller.

Barber provides good accounts of the horrible brutalities of warfare, of the devastations and lootings of the English *chevauchées*—not only were the soldiers greedy for plunder, but also their ravaging destroyed the enemy's economic and military base. Equally good is the account of the decline of Gascony from a financial asset to the English ca. 1330 to a liability after the costly Spanish campaign, when heavy demands for taxation accompanied by local disorders forced extensive concessions from the

government, in particular to retain the loyalty of some of the greater lords.

Barber tries to exonerate the prince from the notorious charge of the wholesale massacre of civilians at Limoges, claiming that it is to be found "only in Froissart's rhetoric"; but he has to admit there is some supporting evidence in Chandos Herald and Walsingham as well as in the French *Chronique des quatre premiers Valois*. The question must remain open. The final, brief chapter of the book contains a discussion of "the Froissart legend." This would benefit from a fuller discussion of the sources for the prince's life as a whole.

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R. R. DAVIES. *Lordship and Society in the March of Wales, 1282-1400*. Oxford: Clarendon Press. 1978. Pp. xv, 512. \$37.50.

The evil reputation of the March of Wales as a disorderly region lying between England and the Principality of Wales as well as the sheer complexity of the march and the very uneven preservation of records relating to it account for much of its neglect by historians. In the present century important work has been done by Goronwy Edwards, William Rees, and others, while a younger generation of scholars has recently begun to remedy the deficiency in a series of doctoral dissertations on individual lordships. The March of Wales is also of general interest to the historian as one of those medieval frontier societies, such as the Iberian peninsula or the lordship of Ireland, that exhibited a fierce independence of central authority, a multiplicity of legal and administrative jurisdictions, and great geographical, social, and economic diversity as well as racial and linguistic mixtures.

Readers of R. R. Davies's articles on the history of medieval Wales have long been aware of his ability to deal with intractable problems and have eagerly awaited the full-scale study to which his essays are a prelude. The promise of his earlier work has been amply fulfilled, since this book is a mature work of meticulously researched and lucidly expressed scholarship that will long remain the standard guide to its subject. The March of Wales existed for nearly five centuries, from the end of the eleventh century until its legal incorporation with England under Henry VIII in 1536. Davies has, however, selected for detailed examination the period from 1282, the year of Edward I's conquest of the Principality of Wales and of the creation of the last of new marcher lordships, until 1400, the beginning of the revolt of Owain Glyndŵr that shook both the principality and the march to their foundations. During the fourteenth century the March

of Wales was, therefore, at its maximum extent and subject to slowly accumulating tensions, so that Davies is able to describe the nature of lordship and its relation to marcher society while avoiding the static picture that studies of institutions often provide. At the end of the book one is forcefully reminded that the marcher lords who loomed so powerfully on the scene in 1282 had by 1400 lost much of their authority to a squirearchy composed of families of Welsh and English descent whose days of glory in local politics were to come after the absorption of Wales into the fabric of the English state under the early Tudors.

In such a wide-ranging book there are bound to be a few quibbles or possible alternative interpretations. A crossreference would have explained the respective interests of the Genevilles and Verdens in the lordship of Ewyas Lacy at the end of the thirteenth century (pp. 260, 282). Was Grimbald Pauncefoot really being punished in 1433 for his neglect of the ancient obligation of castle-guard, or was this retribution for other less easily specified offenses against his lord, the Duke of York (pp. 77-78, 299)?

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University College Dublin

R. A. FLETCHER. *The Episcopate in the Kingdom of León in the Twelfth Century*. (Oxford Historical Monographs.) New York: Oxford University Press. 1978. Pp. x, 288. \$23.50.

R. A. Fletcher establishes the chronological scope of his study to coincide with the period of ecclesiastical reform from Urban II to Innocent III. For León, the closing years of the eleventh century also marked the end of an important phase of the reconquest, the foundation or restoration of several episcopal sees, and the emergence of a new generation of prelates, the most remarkable of whom was Diego Gelmirez of Compostela. Peter Linehan's work, *The Spanish Church and the Papacy in the Thirteenth Century* (1971), provides a convenient reason for concluding the narrative around 1215.

Unfortunately, for this period and place there are basic historiographical problems that render a synthesis such as Fletcher attempts very difficult. The resultant book reflects its origins as a doctoral thesis in a largely unexplored field. Generalizations and imaginative insights are scarce—such oft-repeated phrases as "nothing can be said," "speculation is fruitless," "we know disappointingly little," and "hardly anything is known" typify the narrative of the volume. Reflections on the broader social context of Leonese ecclesiastical history are segregated in an initial chapter. The necessity for establishing

an accurate chronology of bishops in the twelve dioceses under consideration becomes the basis for an entire chapter correcting the standard reference (Pius Bonifacius Gams, *Series Episcoporum Ecclesiae Catholicae* [1873]). Chapter three is strictly paleographical: its extended analysis of diplomatics is valuable in its own right, if disproportionate in a monograph of this sort. One plate and the series of twenty-four original documents printed in the appendix illustrate points made in this chapter.

Chapters four and five on church government and relations with the papacy afford room for genuine interpretation of the sources. This the author attempts in some evocative passages reminiscent of R. W. Southern and through occasional summary statements characterizing the structure and function of the Leonese church. Among Fletcher's principal conclusions are the following: The concept of a metropolitan see, introduced in the twelfth century, failed to lead to such units becoming functional parts of ecclesiastical administration by the end of the century. Disputed metropolitan allegiances and boundaries provoked litigation and a growing habit of recourse to the papal curia. The diocese, on the other hand, with the bishop and his cathedral chapter at its heart, does appear to have been an active force in church government. Chapter life and offices evolved in a manner paralleling that of England, France, and Germany, though with a distinct gap in time; Leonese developments occurred a century or more later than similar developments did elsewhere.

Recognition of papal authority and recourse to it grew apace in twelfth-century León as in the rest of Europe, stimulated by episcopal perceptions of the papacy as a source of privileges, arbiter of disputes, and guardian of reform. Here again, the author gives inordinate space to classifying types of documents, accounting for how many of each are extant for each bishopric, and categorizing types of disputes. There are a few inexplicable lapses in the citation and use of secondary sources, among them that of Alfonso García Gallo's monograph on the Council of Coyanza, *Anuario de Historia del Derecho Español* (1951).

In short, Fletcher's book must be viewed as an initial effort that suggests some directions for comparative church history encompassing the Spanish peninsula, England, and the Continent. His is more an account of what kind of records have survived than of what they can tell us about the Leonese church. Nevertheless, given the state of the archives and a historiographical tradition that has focused for so long on the singularity of the Spanish church, this volume deserves to be welcomed for what it is rather than criticized for what it is not.

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WILLIAM D. PHILLIPS, JR. *Enrique IV and the Crisis of Fifteenth-Century Castile, 1425-1480*. (Speculum Anniversary Monographs, number 3.) Cambridge, Mass.: Medieval Academy of America. 1978. Pp. x, 137. Cloth \$11.00, paper \$5.00.

Biographies of the kings of medieval Castile are scarce, so one must welcome the publication of this study of Enrique IV (1454-74). Arguing that Enrique anticipated many of the policies later developed by Ferdinand and Isabella, William D. Phillips, Jr., attempts to correct the traditional view of him as ineffectual and inept. As an essential preliminary, the author reviews the bitterly prejudiced historiography of the fifteenth century and assesses its influence upon modern historians of the reign. A further discussion of the principal noble families with whom the king had to contend helps to clarify the political situation. Contrary to Suárez Fernández, who thought that the nobles tried to counter royal absolutism with a contractual theory, Phillips believes that they were lacking in any ideology other than that of pure self-interest. This impression is certainly borne out by a reading of the chronicles. In his treatment of Enrique as prince of Asturias and king of Castile, the author adopts a fresh perspective free of the preconceptions of an earlier age. As examples of well-thought-out plans that anticipated those of the Catholic kings, he provides tables based on archival sources that demonstrate that Enrique IV substantially increased the number of academically trained personnel in royal government. Similarly, he tried to control the towns by means of *corregidores* and proposed various economic reforms including a reform of the coinage. By drawing closer to Portugal and England, fostering trade with Flanders, and turning away from France, he adopted policies very much like those of his successors.

As to the charges directed against Enrique by the nobility, Phillips regards as calumny the accusations of islamizing and homosexuality. He upholds the legitimacy of Juana la Beltraneja, chiefly because there is no good evidence against it; but he does not satisfactorily explain the king's abandonment of his daughter and his recognition of Isabella's rights to the throne. If this was done only to gain time and to distract the opposition, it does not reflect an intense paternal love and confirms the judgment of Enrique IV as weak and indecisive. Phillips acknowledges the king's failures and does not attempt a complete rehabilitation. He has convincingly shown that the king did move in certain directions later followed more successfully by Ferdinand and Isabella; yet one is still left with the impression of a king who was unsure of himself, who vacillated (especially in his relations with the treacherous marquess of Villena), and who seemed



incapable of taking the strong measures necessary to bolster his authority.

Even so, Phillips has taken a fresh look at Enrique IV and has based his study not only upon a thorough use of the narrative histories and printed collections of documents but also upon the materials found in the principal Spanish archives. His book can only help to stimulate interest in the turbulent century that preceded the accession of Ferdinand and Isabella.

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JEAN-PIERRE SOSSON. *Les travaux publics de la ville de Bruges, XIV<sup>e</sup>-XV<sup>e</sup> siècles: Les matériaux, les hommes.* (Collection Histoire Pro Civitate, Series in-8<sup>o</sup>, number 48.) Brussels: Crédit Communal de Belgique. 1977. Pp. 375. 750F.

Although building was a basic industry in the urbanized Netherlands, the spectacular development of the textile and metal industries there during the Middle Ages and early modern times has absorbed the main interest of scholars. Only recently has the major building business of sixteenth-century Antwerp received full attention, in a masterly study by H. Soly. Bruges, Antwerp's medieval precursor, gets the spotlight in this doctoral dissertation by Jean-Pierre Sosson, member of the history department of the University of Louvain, who has already given us some remarkable studies on medieval crafts in Brussels and Bruges.

Based on the rich source material of the municipal records of Bruges, Sosson's work concentrates on public works, studying, as the subtitle of the book suggests, the raw materials used and the people involved in it. The region around Bruges was not very rich in raw materials, but the town exploited them very rationally by using the available clay and peat to run its own brickyards as close to the town as possible in order to keep the costs of transport down. The neighborhood south of Bruges supplied some wood, but all of the other commodities were to be looked after by commerce. In this respect Bruges's own market had a lot to offer, but the town authorities nevertheless had to appeal to other centers as well. Dordrecht appears to have been an important market for the slates of Namur (!) and for wood from the Baltic area and from the Ardennes; Zutphen, for wood from the Rhineland. Merchants and boatmen from Kieldrecht, Antwerp, and Mechlin regularly delivered Brabant sandstone to Bruges, whereas Hainault, especially Tournai and Ecaussinnes, normally supplied lime and limestone. Saint-Omer, too, was famous for its lime. Prices of raw materials and wages rose sharply during the

fourteenth century, but leveled off in the fifteenth. The only exception were bricks, whose price kept rising. Let this anomaly stand without further comment.

Employment in the building sector is hard to measure, but at least one-tenth of the active population was involved. The wood workers' crafts were the most important ones; thatchers lost ground from the end of the fifteenth century onward. The corporate organization at Bruges, very much like that of other medieval cities, concentrated political power in the hands of a happy few and imposed great financial and other difficulties upon strangers to prevent them from becoming masters. All this is precisely argued and illustrated by a wealth of facts, assembled in a host of tables and graphs. More revealing is the author's demonstration of the way in which this political ascendancy within the crafts was accompanied by and perhaps caused an economic ascendancy as well.

Corporate rules and craft organization never prevented the formation of small groups of capitalists, monopolizing, as they did, the entire sector of public works. Those masters, whose wages were normally twice those of laborers, got great earnings out of their entrepreneurial and commercial activities. The author is probably right in taking a stand against too great an optimism about the situation of the wage-earners during the later Middle Ages. In my opinion, however, he overdoes it by calculating the bargaining power of the laborers in wheat instead of in the less expensive rye, the common food of the urban masses. The relative decline of real wages during the fifteenth century, after their rise during the fourteenth century, does not mean wage-earners were worse off in the fifteenth century. Graph nineteen shows that, despite a certain decline in most years of the fifteenth century, wages were still above the critical level and that during the fourteenth century, on the contrary, misery was practically permanent.

Sosson has succeeded in writing a perfect scholarly book on a limited subject. At the same time he has made an important contribution to the history of the building industry in the Netherlands as a whole and has revealed the social and economic meaning of the craft system, usually hidden behind its juridical screen.

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JEAN-CLAUDE SCHMITT. *Mort d'une hérésie: L'Église et les clercs face aux béguines et aux béghards du Rhin supérieur du XIV<sup>e</sup> au XV<sup>e</sup> siècle.* (Civilisations et Sociétés, number 56.) Paris: Mouton and École des Hautes Études en Sciences Sociales. 1978. Pp. 264.



We need a solid treatment of the Beguines and Beghards on the Upper Rhine, but this is not it. Jean-Claude Schmitt never decided what kind of book to write. An original or synthetic work? Local or typological? Religious or sociological history? The history of ideas? Or of *mentalités, idéologies, et valeurs*? Within only one hundred and seventy pages of text, Schmitt, an aspiring *annaliste* writing with the imprimatur of Jacques Le Goff, employs none of these approaches at all satisfactorily, much less those of more traditional institutional or narrative history. Thus, save for a few very informative pages (pp. 39–43), the book's readers learn very little about the differences among the Upper Rhenish beguinages, and they are forced to piece together snippets of information about a crucial controversy that culminated in the suppression of the beguinages in Basel between 1409 and 1411. Such irritating features abound.

Like many historians these days, Schmitt has a passion for schemata. Thus, his central argument is that, whereas the Beguines and Beghards were attacked for heresy in the fourteenth century, they were condemned for idleness in the fifteenth. This shift, he maintains, reflects the declining moral position of the Church, the consequent inability of the clergy to raise the cry of heresy, and the clergy's acting instead as the mouthpiece of the newly developing bourgeois work ethic on the eve of the modern era. Neat and interesting, but true? Passing over in silence the question whether this antithesis embraces all of the evidence, we may raise other queries. What of the Brethren of the Common Life, the triumph of whose very similar ideals at the Council of Constance reveals their growing acceptance by the hierarchy? What of Hussitism, a deadly threat to the Church that rightly diverted the attention of clerics and with which they did not fear to deal? Why, as Schmitt admits, were so few Beghards actually convicted of heresy, especially if he is right that it was impossible in theory for the clergy to distinguish between good and bad Beguines (pp. 96–114)? And why were they accused of idleness when many of them earned at least part of their income? Schmitt does not answer these questions, although he inadvertently hints at one important clue in stressing the strong ties between the Beguines and the mendicants, especially the Franciscans, in whose bitter struggles with the secular clergy the Beguines easily became embroiled but from whom they also received protection.

This last issue points both to another polarity running through the book, that of the clergy versus the laity, and to Schmitt's implicit anticlericalism. He recognizes the frequently acrimonious disputes within the clergy in the late Middle Ages. He underscores correctly, but hardly for the first time,

how inconvenient churchmen found the Beguines for any traditional conception of *ordo* and how clerical opposition to the Beguines waxed steadily in the fourteenth century. But the attitude, much less the conduct, of most churchmen was far more complex than Schmitt implies. So, too, were their motives. They were troubled not only by abstract ideas about order but also by practical problems of discipline as well. If bishops faced enormous obstacles in supervising the clergy proper, how could they prevent those who lay in the penumbra of their jurisdiction from going astray and giving scandal? The gravity of the problem intensifies considerably if Schmitt is correct in his apparent suggestion (as usual, insufficiently explored) that many Beguines and Beghards had taken up this state of life for less than pious reasons. Whether or not this was so, we must avoid a facile modern tendency to assume a clerical monopoly on depravity and a lay monopoly on true piety. When in 1425 Bishop Raban von Helmstadt of Speyer (a diocese which Schmitt curiously omits from his discussion, perhaps because its five Beguine houses do not conform to his conceptions) proceeded against the house of Beguines in Weissenburg because of its putative corruption, he may just possibly have been telling the truth and acting sensibly, not simply because he was obsessed with order and could not possibly discriminate between good and bad Beguines.

Another facet of Schmitt's anticlericalism is revealed in his subtitle and his choice of material for discussion, which could lead one to infer that the Beguines constituted a puzzle only for the clergy. This, too, was not the case. Elsewhere in northwestern Europe the Beguines' economic activity (not, be it noted, their idleness) aroused the stiff opposition of burghers. Schmitt alludes to comparable difficulties on the Upper Rhine, but he does little more than that, presumably because these facts do not conform to his *ordo*. This kind of disturbing omission causes one to wonder not only about possible lay pressure leading to the expulsion of the Beguines from Basel but also about the overall fairness of Schmitt's treatment.

In sum—despite strengths, like a moderate amount of information, many sensible observations, and some stimulating speculations (for example, on the origins of the “work ethic” in the manpower shortage caused by the Black Death [pp. 191ff.])—this book is too long on language, theory, and models—in short, on oversimplification—to serve the historian of the late Middle Ages very well.

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JOHN MARTIN KLASSEN. *The Nobility and the Making of the Hussite Revolution*. (East European Monographs,

number 47.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1978. Pp. 186. \$12.00.

John Martin Klassen correctly states in his introduction that until now there has been no study of the role of the nobility in the Hussite revolution. Although his book has significant deficiencies, it should, nevertheless, be valuable to scholars of the later Middle Ages because it is a pioneering work in a difficult field of research. Klassen's main thesis is that during the Hussite revolution the nobles had "no unified approach to the issues of church reform" and that "the political achievements of the revolution fell to them by default" (p. 1). At the same time, however, he believes that the nobility had some sense of identity and a generally unified set of political goals. The major part of his own research has involved noble church patronage in Bohemia and how it was used by the nobles of the various parties during the Hussite revolution.

The book is quite uneven in its quality. Following an interesting and valuable sketch of Bohemia's economy and society on the eve of the Hussite movement are several chapters on patronage and the political machinations of the fourteenth century. In these Klassen tends to draw excessive or, at best, highly conjectural conclusions from the data. For example, can it be assumed that unexplained resignations from benefices were exacted by the patrons (p. 32) or that 10 percent less noble patronage in the territories of Hussite nobles suggests a major reason for these nobles' choice of the Hussite side (p. 43)?

When Klassen tries to explain theological and philosophical issues, he gets bogged down by attributing too much importance to the political theories of John Wyclif and even has the annoying habits of referring to "the Wyclifite program to reform the church of the Bohemian realm" (p. 81) and of calling the entire Hussite movement in 1428 "Wyclifite" (p. 85). Klassen's failure to understand the religious elements of the Hussite movement casts doubt on his later suggestions about the relationship between belief and political and economic self-interest.

Klassen is at his best in chapters eight and nine, devoted to Hussite and Catholic politics and patronage in the crucial years 1415 to 1419. Here he draws some significant conclusions: (1) that Hussite nobles only began to turn out Catholic clergy in 1417 as a reaction to pro-Catholic actions by the king and the archbishop of Prague; and (2) that the Catholic nobles, while supporting the Catholic Church, often took over property rights from the clergy.

Generally speaking, Klassen's book is a useful

one, especially since it is the only one on the subject. Klassen sometimes draws conclusions in excess of his data, lacks tight organizational skills, and has a simplistic understanding of the religious issues. His account also could have sometimes benefited from the use of parallel studies from other parts of Europe to try to fill some gaps. On the other hand, he has carefully studied patronage (providing useful graphs and charts) and has given us some new and valuable insights into the critical years 1415-19.

WILLIAM R. COOK

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HANNA ZAREMSKA. *Bractwa w średniowiecznym Krakowie: Studium form społecznych życia religijnego* [Confraternities in Medieval Cracow: A Study of the Social Forms of Religious Life]. Summary in French. Wrocław: Ossolineum. 1977. Pp. 190. 45 Zł.

The sociology of religion is a field now three-quarters of a century old. In treating the period of the Middle Ages, Gabriel Le Bras made many fundamental contributions that have been widely influential. His work has found a particularly favorable response in Poland, where a productive circle of scholars, many working in conjunction with the medieval culture study group of the Polish Academy of Sciences' Institute of History, has pursued this field with considerable success. Hanna Zaremska's book, originally written as a doctoral dissertation within this institute, makes a useful contribution to the study of religious confraternities in the late Middle Ages by focusing upon these groups and the religious role of the laity in fourteenth- and fifteenth-century Cracow. She has successfully exploited the extensive archival materials on this subject that are contained in the state, academic, and ecclesiastical repositories there. In addition, she has used a wide selection of printed sources.

Her study is, in effect, divided into two sections. In the first, which focuses upon the external aspects of the groups under discussion, she begins with a general sketch of the ecclesiastical and social structure of Cracow in the late Middle Ages. This is followed by a chapter that describes the institutional and organizational form of the fourteen confraternities associated in this period with churches in Cracow and its two suburbs of Kazimierz and Kleparz. The next chapter analyzes the process by which the membership of these groups was recruited. This part of the book concludes with a chapter that provides a social profile of these members. Throughout these chapters, Zaremska's scholarly touch is sure.

The last section of the book, consisting of three

chapters, deals with the internal life of the confraternities. The author discusses the character of their religiosity and the practice of piety that they furthered. She analyzes participation in church festivals, in burials, and in processions such as those associated with Corpus Christi day. A final chapter is devoted to an evaluation of the role of women and family groups within the confraternities and stresses the occupational, residential, and even ethnic ties that bound individuals together. A short conclusion ends the book. This second part of the study provides some excellent insights. For example, the prominent role played by women, particularly in positions of leadership, suggests an aspect of their independence that is not traditionally seen in medieval society. Also, the growing variety of processions and other public religious expressions that involved the confraternities reinforces the emerging scholarly picture of dynamic popular piety in this period. In this section, however, the data used by the author are by their nature more impressionistic than those of the first section and less capable of providing the basis for rigorous and precise interpretations. Consequently, the author's points are less surely made. Despite this shortcoming, this study of a particular kind of popular piety in the late Middle Ages is a welcome contribution to the literature on the sociological aspects of this subject.

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## MODERN EUROPE

PETER BURKE. *Popular Culture in Early Modern Europe*. New York: New York University Press. 1978. Pp. 365. \$20.00.

In these nine self-standing but related essays, Peter Burke explores the content, significance, and development of popular culture in Europe during the period 1500–1800. In addition to being broadly based (examples are cited from Dalmatia to Scotland, Portugal to the Ukraine) and synthetic (anthropologists, ethnologists, and art historians all have their say), this study is also a joy to read.

Beginning with an excellent chapter on eighteenth- and nineteenth-century “Discoverers of the People” (Jacob Grimm, J. G. Herder, and others), Burke then scrutinizes the major distinctions within popular culture: rural-urban, male-female, regional, linguistic. He differentiates between the roles played by wanderers (soldiers, sailors, beggars, thieves) and professional “performers” (charlatans,

mountebanks, puppeteers) in the diffusion of popular culture from that played by “amateurs” (colporteurs, cunningmen, itinerant tailor-singers, Calabrian funeral “howlers,” and so on). He examines formulae commonly appearing in songs, dances, and paintings throughout Europe. Through example and analyses of the relevant ideas of Redfield, Propp, Lévi-Strauss, and others, Burke shows that cultural motifs are modified by living, breathing women and men, rather than existing in some sort of theoretical never-never land.

In “The World of Carnival” and “The Triumph of Lent” (chaps. 7 and 8), Burke shows how laity-led reforms of the period 1650–1800 (“a reformation within the reformation [both Catholic and Protestant]”) differed from the preceding century and a half’s clergy-led reforms (p. 239). To this point, Burke has described and analyzed popular culture on its own terms; his final chapter attempts to describe and analyze the impact of three centuries of economic, political, and social change on popular culture. He concludes that “the commercial revolution led to a golden age of traditional culture (material culture at least) before the combined commercial and industrial revolutions destroyed it” (p. 246). By the nineteenth century, the literate, powerful, and comfortable had withdrawn from popular culture and “abandoned it to the lower classes” with whom, three centuries before, they had been happy to share it. Only at the time of that withdrawal, paradoxically, did popular culture become “discovered” by scholars and collectors (Burke ends his story with the same Grimms and Herders with which his book begins).

The book includes excellent notes, a twenty-one page bibliography, and sixteen pages of illustrations. Specialists and nonspecialists, teachers and students should welcome Burke’s book. It deserves the widest audience.

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ANDRÉ CORVISIER. *Arts et sociétés dans l'Europe du XVIII<sup>e</sup> siècle*. (L'historien, number 34.) Paris: Presses Universitaires de France. 1978. Pp. 244.

André Corvisier is Professor of Modern History at the Sorbonne. Although his previous work has been mainly on the relationship between war and society, he appears to have had a long-standing interest in eighteenth-century art, thanks to the influence of Louis Réau and Victor-L. Tapié. He is not the first historian of the *Annales* school to write on art—Georges Duby has also done so. As fine as Duby’s books are, they are part of a series whose focus is on art, ideas, and history and do not represent a full-

scale application of the *Annales* approach. The book under review here does constitute such an effort.

It is divided into five parts and includes a total of sixteen chapters, all of which are subdivided into smaller units. That a book of 230 pages should have so many components is consistent with the author's attempt to survey systematically the relationship between eighteenth-century art and society. The book examines subjects such as patronage, the social background of artists, the role of guilds and academies of art, the connection between taste and social groups, and how art reflects various *cadres*—religious, monarchical, princely, noble, bourgeois, administrative, and popular. Corvisier does not limit himself to the fine arts but also discusses the “artisan arts,” as practiced by cabinetmakers, ceramists, engravers, and the like. He also includes music, but not literature; the emphasis is on France, but the book does survey all of Europe.

The book's scope is obviously vast, and given its modest size it is necessarily schematic and one dimensional, as if it were a blueprint rather than a fully realized structure. Its value lies in a solid understanding of eighteenth-century society and its institutions and a skillful examination of both through art. As a historian Corvisier uses art to shed light on eighteenth-century life, showing that the artist's perception conveys something of the style, quality, and dynamics of society, within the framework of a stratified, hierarchical order. In the capacity of art historian he shows how the artist's training, conditions of employment, subjects, social acceptance, and mentality reflect the conventions, traditions, and class realities of eighteenth-century life. While the approach is entirely consistent with that of the *Annales* school, the book does not have the rigor that one associates with it. He has done no quantifying himself, which is clearly necessary if many of the questions that he asks are to be answered. When, as in a few cases, he is able to draw on the work of others, the results are certainly valuable. Perhaps the book's greatest value lies in its indicating the many areas of research that need to be done and that could be undertaken either by historians or art historians or, even better, by teams of both.

One of Corvisier's underlying assumptions may be questioned: that there was an *embourgeoisement* of aristocratic and even monarchical art during the eighteenth century. He himself offers ample evidence that the establishment had an immense capacity to assimilate artists up to the very end of the *ancien régime* and indeed that art, if anything, became more exclusive.

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GEDALIA YOGEV. *Diamonds and Coral: Anglo-Dutch Jews and Eighteenth-Century Trade*. New York: Holmes and Meier. 1978. Pp. 360. \$21.75.

Gedalia Yogev's book overcomes the difficulties inherent in writing about eighteenth-century Jewish economic history. Such a specialized subject might more easily have been studied by adopting a narrow focus, but Yogev has chosen to make this account of the activities of Jewish merchants useful to a larger audience of historians by placing it in the context of eighteenth-century business history. The first of the three sections of the book provides a useful general survey of the activities of Jews in the trade with India, the West Indies, and America and with the Spanish and Portuguese empires. The description of the not inconsiderable role of Jews in these areas of trade not only provides a reliable account of the business of Jewish merchants, but it also contributes useful information about the operation of these important areas of business activity.

The second section of *Diamonds and Coral* focuses on Jewish activity in a single trade, diamonds. It is impossible to establish precisely the relative importance of the various causes of the increase in the demand for diamonds that appears to have begun in the latter part of the seventeenth century. An increase in the supply of diamonds in Europe caused a decrease in prices, which made diamonds more readily affordable by Europeans who were increasingly prosperous, and who, especially among the emerging middle class, sought to purchase luxury goods that also might be considered a good investment. England's near monopoly in Indian diamonds made London a major supplier of diamonds for Germany, France, and Holland.

Jewish merchants were especially well placed to engage in the diamond trade. Because Indians valued coral highly, it was possible for merchants in the diamond trade to comply with English restrictions on the exportation of precious metals by purchasing coral in Leghorn for sale in India. The personal and family connections of Jews recently settled in England with Jews trading in Leghorn gave English Jews an advantage that, after the first Jewish agent arrived in Madras in 1686, allowed them to participate at every stage of the diamond business, from the time coral was purchased in Leghorn for shipment to India until the diamonds were cut, polished, and retailed.

The Anglo-Indian diamond trade began a twenty-five year period of decline during the 1760s. When the East India Company restricted the amount of money that could be sent to England by drawing bills on the company that were payable in London, coral sellers were forced to purchase diamonds to transfer money home. This restriction



created a seller's market for diamonds in India. The problem was exacerbated by the increase in the number of Englishmen who attempted to transfer their fortunes from India to England by purchasing diamonds for resale in England, thus flooding the European diamond market.

The third section of *Diamonds and Coral* is an account of a single Jewish firm, Prager Brothers, from 1760 until the collapse of the London branch of the business in 1796. Based on the extensive correspondence among the London, Amsterdam, Ostend, and Philadelphia branches of the business, the letters not only provide an excellent account of Anglo-Dutch trade in diamonds, tobacco, and exchange transactions, but they also reveal much about the personal and family life of a prosperous Jewish family.

*Diamonds and Coral* is solidly based on extensive archival research. Yogeve judiciously evaluates the historical importance of the source material and resists drawing conclusions that go beyond the extent of the evidence. *Diamonds and Coral* is an important book for historians of eighteenth-century Jewish economic history; the breadth of Yogeve's research makes this study generally useful to scholars studying other aspects of eighteenth-century economic and social history as well.

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ALBRECHT HIRSCHMÜLLER. *Physiologie und Psychoanalyse in Leben und Werk Josef Breuers*. (Jahrbuch der Psychoanalyse. Beiheft, number 4.) Bern: Verlag Hans Huber. 1978. Pp. 461, 18 plates. 56 Fr.

Increasingly, we are learning of the variety and complexity that characterized late nineteenth-century science. It is no longer accurate to depict it as a mechanistic-materialistic monolith. Albrecht Hirschmüller's biography of Josef Breuer, the physiologist and internist who was Freud's early collaborator, provides further evidence that science and scientists cannot be conveniently cached under tidy labels.

In a convincing fashion, Hirschmüller shows us a Breuer whose fundamental outlook was not that of mechanistic, positivistic science, although he was trained in this tradition. Breuer expressed himself in the language of this science and skillfully used its methods, but he was at heart a vitalist and teleologist, influenced more by Hering's nativism than by Helmholtz's empiricism.

Viewing Breuer in this way enables Hirschmüller to modify some—though definitely not all—traditional psychoanalytic assessments of Breuer's contributions to the origins of psychoanalysis. Hirsch-

müller does not have the evidence he thinks he does for a thorough revisionist account. What Hirschmüller can effectively do is show that Breuer's work as a psychopathologist is not a puzzling anomaly in an otherwise understandable scientific career. Rather, we see how Breuer's patience, experience as a practicing physician, and expertise in observation led to his long and intensive treatment of Anna O. We discover how his interest in physiological regulatory mechanisms, his conviction of the determinism of psychic events, and his belief in the biological meaning and goals of the body's mechanisms were in accord with his theoretical conclusions about hysteria. Hirschmüller has grouped Breuer's work in psychopathology into three time periods, thus providing an exceptionally clear elucidation of Breuer's contributions as well as a broad understanding of the theoretical grounds over which Breuer and Freud broke. Hirschmüller contends that, more than anything, Breuer objected to Freud's energetic model of how the mind worked, an explanatory schema in the tradition of nineteenth-century mechanistic (and speculative) brain mythology.

With the aid of new sources, Hirschmüller has shown that Anna O. was not the first or only psychiatric patient Breuer ever treated and that Breuer continued to treat emotionally disturbed patients after Anna O. But, try as he may, Hirschmüller cannot evade the significance of the fact that, after Anna O., Breuer never again used the cathartic method. And, although Hirschmüller successfully follows in the footsteps of Henri Ellenberger, destroying some of the myths regarding the Breuer-Anna O. relationship, he fails in his attempt to bring down the traditional view that Anna O.'s transference and Breuer's countertransference figured heavily. Moreover, Hirschmüller deals poorly with an important question he himself raises. Hirschmüller establishes beyond a doubt that Breuer *knew* that Anna O. remained ill for many years after his treatment of her. But why, then, in *Studies on Hysteria* did Breuer give the impression he had cured her? Hirschmüller's speculations are curiously weak. Nevertheless, on the questions of why Breuer and Freud wrote the "Preliminary Communication" ("On the Psychical Mechanism of Hysterical Phenomena") and why Freud wrote the *Project for a Scientific Psychology*, Hirschmüller's speculations are quite provocative.

Although much of the material in this book that pertains to the pre- and early history of psychoanalysis can be found elsewhere, it is helpful to have it all brought together. And there is some new information dealing with Anna O.'s long illness and eventual recovery. Hirschmüller's major contribution is the new perspective he offers on Breuer's own



life and preoccupations, buttressed by a lengthy appendix containing previously unpublished correspondence. His interpretation of Breuer's career prompts me to propose that Breuer and Freud were drawn together not merely by Freud's monetary and emotional needs and Breuer's kindly attempts to help a promising younger colleague but their shared belief that a human being cannot be reduced to mechanistic categories.

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VASA ČUBRILLOVIĆ, editor. *Velike sile i Srbija pred Prvi svetski rat: Zbornik radova prikazanih na međunarodnom naučnom skupu Srpske akademije nauka i umetnosti, održanom od 13–15. septembra 1974. godine u Beogradu* [The Great Powers and Serbia before the First World War: A Collection of Works Presented at the International Symposium of the Serbian Academy of Sciences and Arts, held from 13 to 15 September, 1974 in Belgrade]. (Naučni skupovi, number 4; Odeljenje istorijskih nauka, number 1.) Belgrade: Srpska Akademija Nauka i Umetnosti. 1976. Pp. xxiv, 843.

In 1964 four of Yugoslavia's scientific academies decided to commemorate the beginning of the Great War with an anthology of topical articles. When one compares the papers of that earlier effort (published by the Serbian Academy of Sciences in 1967 under the title *Jugoslovenski narodi pred Prvi svetski rat* [The Yugoslav Peoples Before World War I]) with the thematic collection at hand, it is plain that the present volume is not as successful.

Unlike the 1967 symposium, which was also edited by Vasa Čubrilović, the papers of the conference on "The Great Powers and Serbia before the First World War" represent an international effort. In addition to twenty-two scholars from Yugoslavia (thirteen of them from Belgrade), twenty-eight foreign specialists (from Bulgaria [six], Austria [five], U.S. [four], Poland [three], Rumania [three], USSR [three], DDR [two], BRD [one], and Czechoslovakia [one]) also participated in the Belgrade meeting, and their papers are reproduced in this collection. Nevertheless, it would not be correct to assume that this multilateral effort reflects the state of international studies on Serbia's antebellum relations with the Great Powers. The choice of topics for many of these articles is quite capricious, and some of the papers by historians who ordinarily do not work on the 1878–1914 period evidently were concocted to suit the overall theme and meet the deadline. Moreover, a few articles are simply boring exercises, utterly arid and lacking in interest.

The section devoted to "The International Work-

ers' Movement on the Causes and the Beginning of World War I" is a good example of the organizers' indulgence. With a few exceptions, the authors of the fourteen reverent articles in this subdivision hold forth at length on matters that are already well known (the views of Dimitrije Tucović and Lenin) or that should have been left in obscurity (the International's antiwar demonstrations in November 1912). But even the excessive solicitude for ideational predecessors could not be faulted, had the editors encouraged analysis of competing, or simply different, political movements and groups. As it stands, only Liubomir Ognianov, in an article on the Bulgarian *Zemledelski sŭiuz*, recognizes the existence of various nonsocialist parties, the decided political majority in Serbia and throughout the Balkans.

Although quite unbalanced, the collection includes some useful contributions. In his introductory article, Čubrilović provides a fine summary of the mutable relations between renaissance Serbia and the Habsburg Monarchy from 1804 to 1914. The *personae* of a historian and a participant in the Young Bosnia movement have converged in this effort, permitting Čubrilović's adroit display of orthodox Belgrade views on matters Austrian. Equally useful are the contributions of some other participants, notably Imanuel Geiss, Richard Georg Plaschka, and Dimitrije Djordjević, who on the whole restate their influential earlier findings. One must also single out Traian Stoianovich's original interpretation of Serbia's growing spatial-economic independence as a source of conflict with Vienna. Two other articles, by Willibald Gutsche and Tsvetana Todorova, on the economic aspects of the pre-1914 Balkan crises are also quite illuminating. Peter Sugar's review of American newspaper reaction to the 1914 assassination crisis stands out among several articles on international public opinion.

One useful avenue that the conference should have investigated concerns Serbia's activist, indeed irredentist, policies in the Dual Monarchy. Several authors play ducks and drakes with this issue, but only Milorad Ekmečić and Arnold Suppan seize upon it, although not without revealing their respective ideological sleights. Much work remains to be done on the antebellum South Slavic politics before we rid ourselves of conspiratorial bugbears. Historiographic sanction notwithstanding, most plots have loose ends.

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V. K. VOLKOV *et al.* "*Drang nakh Osten*" i narody Tsentral'noi, Vostochnoi i Iugo-Vostochnoi Evropy, 1871–1918 gg. [The "Drang nach Osten" and the Peoples of

Central, Eastern, and Southeastern Europe, 1871–1918.] Moscow: Izdatel'stvo "Nauka." 1977. Pp. 316. 1 r. 60 k.

*Drang nach Osten* is a volume designed to restate standard Soviet interpretations of the history of German imperialism and of World War I, albeit through extensive use of recent Western sources.

The critique of German imperialism uses many of the arguments advanced by Fritz Fischer in his works on the subject, arguments reinforced by repeated quotations from less objective students of imperialism including Marx, Engels, and Lenin. A few variations on the anti-German theme, unique to the authors of the present study, are worthy of further comment. One is the stress on the intimate relationship between German and Austro-Hungarian imperialistic interests in Eastern Europe and the other is assigning to the Balkans the role of the epicenter of German imperialism. Although these interpretations are not altogether novel, they are significant in that they stress the continuing importance attached to East Central Europe as an area of vital concern to Russia in its historic struggle against German revanchism and Western imperialism. The customary indictment of German militarism during World War I is devoid of originality and denies, once more, the existence of any organic relationship between Germany's promotion of Lenin's return to Russia and the outcome of the war and the course of the revolutionary movement in Russia. In fact, the inevitability of the victory of the Bolsheviks and the corollary liberation of all peoples of East Central Europe from the permanent threat of German imperialism are themes stridently reiterated throughout the book.

The bibliography is unimpressive and the use of primary sources is minimal. Its political message and implications aside, the present volume has the merit of being one of the pioneer studies of the *Drang nach Osten*. An objective and properly documented volume on that subject, however, remains to be written.

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WERNER ZÜRRER. *Kaukasien, 1918–1921: Der Kampf der Grossmächte um die Landbrücke zwischen Schwarzem und Kaspischem Meer*. Düsseldorf: Droste Verlag. 1978. Pp. 733. DM 78.

Focusing on Georgia, Armenia, and Azerbaidzhan during their brief separation from Russia during and after World War I, Werner Zürer examines the *Kaukasuspolitik* of Germany, the Ottoman Em-

pire, Great Britain, and Soviet Russia (occasionally mentioning the U.S., France, and Italy) in the context of the Russian "question." Using new German and British Foreign Office materials and available Soviet sources, Zürer, who has published on related topics, presents an informative, judicious, and generally persuasive assessment of the powers' attempts to shape the future of strategically important Transcaucasia. In so doing, he explains why they failed and why Soviet Russia succeeded.

Excepting Great Britain's Baku venture, the first chapters detail Turkish expansionism and growing German interests (primarily economic and strategic, not imperialistic, says Zürer), particularly in Georgia, which clashed with Turkey's aims. German authorities, however, failed to agree on a course of action. This disunity, concern about relations with Soviet Russia, Turkey's presence in the area, and preoccupation with European developments prevented Germany from playing a more effective role.

The Central Powers' collapse opened the door to Great Britain. Attempts to facilitate a settlement were complicated by basic disagreements among authorities, torn between supporting either the centralist or the centrifugal forces in Russia and exacerbated by the sharpening of local conflicts, General Denikin's threatening moves, the presence of Turkish forces, and a weakening of "imperial" commitment. Great Britain's decision to withdraw from Russia applied to the Caucasus as well. Zürer's analysis of Great Britain's decision-making process during its last phase of imperialism is disappointing. For example, he fails to examine fully Lloyd George's role.

Halfhearted efforts to deal with Transcaucasia at Paris, or involving other powers, failed. Foreign political and material aid was indispensable for Transcaucasia, wedged between resurgent Soviet Russian and Turkish expansionism. Sufficient help did not come and the always precarious prospects for independence waned quickly. Soviet Russia met no serious obstacle. Whether or not Zürer's criticism of Soviet imperialism will elicit responses from Soviet scholars, whom he would like to shed light on still obscure details, remains to be seen.

Reading this book is difficult: one-third consists of frequently long notes in the back. There are too many exclamation points and quotations. Too many characters, identified as diplomats or officers, are introduced only in the footnotes: a good subject index and a complete index of persons would have been valuable. The single map is inadequate; several maps depicting shifting frontiers and economic and demographic details would have been useful.

The documentation has significant gaps: in discussing Turkey's role, Zürer relies almost exclusively on non-Turkish sources; the Lloyd George pa-

pers were not used; only published U.S. sources are mentioned; and periodical literature is neglected.

The author laments the loss of independence of Georgia and Armenia in particular. He scores Turkey's chimerical panturanianism and Soviet expansionism, criticizes Great Britain's lack of foresight and resolve on behalf of the republics, and generally deplores the West's vacillation and indifference to Transcaucasian desires. He does not absolve the republics from blame, noting their inability to cooperate against Turkey and Soviet Russia, their quarrels with Denikin, their conflicts over territories, their traditional animosities toward one another, their ineffectiveness in presenting their case to the world, their internal weaknesses, and their lack of determination in remaining independent.

The book supersedes Firuz Kazemzadeh's work on the struggle for Transcaucasia, while adding important amplifying and clarifying information to the discussion of German and British policy by Fritz Fischer (with whom the author disagrees occasionally), Winfried Baumgart, Oleh Fedyshyn, and Richard Ullman. Zürer's monograph is now the best available account and is a solid addition to the growing literature on intervention in Russia.

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YEHUDA BAUER. *The Holocaust in Historical Perspective*. (The Samuel and Althea Stroum Lectures in Jewish Studies.) Seattle: University of Washington Press. 1978. Pp. ix, 181. \$8.95.

FALK PINGEL. *Häftlinge unter SS-Herrschaft: Widerstand, Selbstbehauptung und Vernichtung im Konzentrationslager*. (Historische Perspektiven, number 12.) Hamburg: Hoffmann and Campe. 1978. Pp. 337.

Here are two contributions of a very different sort to the literature of the Holocaust: one a collection of essays that reflects on the meaning of the Holocaust; the other a specialized study of concentration camp behavior. Yehuda Bauer's essays, presented initially as lectures at the University of Washington, seek to clarify a perspective from which to evaluate the vast Holocaust literature and to point to work that still needs to be done. Falk Pingel's monograph, originally a dissertation at Bielefeld University, attempts to sort out the concentration camp conditions that enabled certain groups of prisoners to circumvent psychological demoralization and mount, in some instances, resistance and even rebellion.

Of the two, Bauer's work is the more useful. As the author of numerous works on Jews and the Holocaust and head of the Institute for Contemporary Jewry at the Hebrew University in Jerusalem, his

reflections are as important for the expert as for the novice. His essay "Against Mystification" should be read as a point of departure by anyone trying to come to grips with the Holocaust. Its reality, Bauer argues, has been obscured not only by those who deny it ever happened and by revisionists like David Irving, who Bauer fears are creating the conditions for rehabilitating Nazism, but also by those who abstract the Holocaust and envelop it in a "nebulous general humanism, where all persecutions become holocausts . . ." (p. 3). Even to a century that must catalog among its horrors massacres of Armenians, Kurds, and Sudanese blacks, Bauer rightly finds Nazi efforts to exterminate an entire people unique. The uniqueness he locates in an analytical distinction between genocide and Holocaust. Genocide was a term invented in 1943 to describe Nazi policy in Eastern Europe *before* knowledge of the Final Solution was widespread. It could, and did, include murder of elites who might provide cultural leadership for a people; it certainly included robbing them of their moral identity and led to their enslavement. But genocide did not mean, as Holocaust did, "wholesale, total murder of every one of the members of a community" (p. 35). There have been numerous genocides, but only one Holocaust.

In an equally important essay on "Jew and Gentile," Bauer takes a *tour d'horizon* of the Gentile world during the Holocaust to assess that world's response, concluding that most generalizations remain unsupported by adequate evidence. Even in Eastern Europe, where the response of Gentiles ranged mostly from indifference to active participation, there is the exception of Bulgaria where, despite strong anti-Semitic traditions, most Jews were saved. For the churches generally there is both moral failure and heroism, although minority churches seem to have displayed more backbone than established ones. Bauer's tour includes Great Britain, the United States, and the neutrals and demonstrates in each instance that there is much still to be learned.

A final essay on the 1944 offer of the SS to trade Hungarian Jews for trucks concludes that Joel Brand, who carried the offer to Istanbul, was unwittingly the front behind which Himmler was trying to make contact with the West in hopes of securing a separate peace for the crumbling Reich. Although he is unable to trace the connection all the way to Himmler's desk, Bauer's case is a persuasive one, and his essay brings to our attention a great deal of work that has appeared in Hebrew. The true failure in this affair, Bauer contends, was the West's refusal to negotiate when negotiations, by delaying the transports to extermination camps, might have saved lives.

Falk Pingel, intending to go beyond the insights

of the likes of Bettelheim and Frankl into the behavior of individual concentration camp prisoners, constructs a model he hopes will shed light on the behavior of groups of prisoners. His model attempts to relate group behavior to changes in the political and economic circumstances of the Reich that were ultimately reflected in changing concentration camp functions. These he sees as providing the Reich from 1933–36 with political security, from 1937–41 with cheap labor, and from then on, also, with stations for implementing the Final Solution. Changing functions resulted in changing camp conditions and therefore, Pingel argues, supported changing modes of survival and resistance to the relative advantage, in turn, of different prisoner groups. Much that is valuable in this study is obscured rather than illuminated by the complicated theoretical structure.

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YISRAEL GUTMAN and EFRAIM ZUROFF, editors. *Rescue Attempts during the Holocaust: Proceedings of the Second Yad Vashem International Historical Conference, Jerusalem, April 8–11, 1974*. New York: Ktav. [1978]. Pp. 679. \$17.50.

The present volume contains the lectures and debates of a conference held in Jerusalem in 1974 at Yad Vashem, the central repository for Holocaust documents. The participants, Israeli scholars for the most part, generally reflect the institution's commitment to meticulous historical research and its avoidance of glib generalization and moralistic musing. The central question raised in the twenty-one essays and in the debates—why so few governments and individuals aided European Jewry during the Holocaust—has long troubled historians of the Final Solution. The fact that the conference was held only six months after the Yom Kippur War lent an added urgency and intensity to the discussion.

The conclusions to be drawn from a careful reading of the essays are grim and troubling. Most governments were informed of the dimensions of the Final Solution as early as mid-1942, yet few took any concerted action to rescue Jews. The reasons were many and complex: duplicity on the part of anti-Semitic officials, confusion over what was believed to be conflicting and inadequate information, disbelief in the face of the enormity of the crimes, domestic political infighting and interstate rivalries, the weighing of priorities that placed narrow self-interest above the rescue of stateless civilians, and so on. For those who still cling to a belief

in the willingness of Western democracies to aid refugees, the essays reveal that there was little substantial difference between the relief efforts of the Allies and those of the occupied countries of Eastern Europe.

The proceedings also shed light on the inadequacy and infrequency of individual acts of rescue. Many apologists in Poland and Germany argued after the war that rescuing Jews would have endangered their lives, yet a number of the essays show that humanitarian acts were not always punished by Nazi occupiers. More importantly, the failure of European civilians to engage in rescue operations during the Holocaust was only one element in the tragic continuum of apathy toward the Jewish plight that extended from the lukewarm reception of German refugees in the West after 1933 to the indifference toward stateless displaced persons after World War II.

Rescue, therefore, was generally a question of scattered individual effort, either by enterprising Jews who saved themselves or attempted to save others or by "Righteous Gentiles," valiant non-Jews who hid Jews or helped them to flee Nazi-occupied areas. As Yisrael Gutman points out in his essay on Polish attitudes toward the mass deportations of Jews, however, escape to areas under the control of anti-Nazi forces did not always ensure safety. Many Jews who fled to the countryside to join Polish partisans, for example, were either shot or turned over to Nazi officials.

Though the general quality of the presentations is high, the volume reflects a number of problems that are all too common in Holocaust historiography. In their attempts to avoid the antihistorical moralizing of individuals such as the late Hannah Arendt, many Holocaust scholars have tended to shy away from analysis completely in favor of documentation and simple narrative. Thus the majority of papers fill in gaps of knowledge concerning specific events and personalities relevant to the issue of rescue during the Holocaust, but only a few combine factual presentation with critical insight. Notable exceptions are the essays by Leni Yahil on the rescue of Danish Jewry, which attempts to isolate the various internal and external factors that determined the nature and extent of rescue efforts, and by Moshe Bejski on the "Righteous among the Nations," which attempts in part to determine what led individual non-Jews to aid Jewish victims.

A closely related problem typical of all contemporary history is the inevitable conflict between the active participant in the event and the historian. Though the debates are often quite helpful in clarifying issues raised in the papers, there are a number of occasions where the discussion degenerates into a *dialogue des sourds* between survivors seeking to de-



fend their behavior during the war and Holocaust scholars. It is interesting in this context to note that one of the more successful presentations is delivered by an American historian, Henry Feingold, whose discussion of Roosevelt and the resettlement question is both skillful and far enough removed from the concerns of other participants to avoid being mired in acrimonious debate or self-justification.

Despite these problems, the volume is a valuable source for students of the Final Solution. Yad Vashem can take justifiable pride in organizing a conference that demonstrates that even a controversial and emotion-laden subject such as rescue during the Holocaust can be discussed by historians with dispassion and intelligence.

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JØRGEN HÆSTRUP. *Europe Ablaze: An Analysis of the History of the European Resistance Movements, 1939-45*. (Odense University Studies in History and Social Sciences, number 55.) Odense: Odense University Press. 1978. Pp. 557. 120 KR.

With this work, Jørgen Hæstrup joins a select group of historians—including Henri Michel, Heinz Kühnrich, and Henri Bernard—who have attempted to write a comprehensive account of Resistance movements throughout occupied Europe from 1939 to 1945. Hæstrup excludes from his study only those countries that formed part of the Axis alliance, with the exception of Italy after 1943.

The book begins with a "survey of problems" that historians of the European Resistance must confront, among which is the difficulty of accurately assessing the importance of the contribution of much Resistance activity to the overall war effort. But the author's concluding summary is optimistic in this regard. He asserts confidently that the Resistance movements as a whole "came to exercise a not unimportant influence on the course of the war—also militarily" and that in some countries, such as Italy, France, Yugoslavia, and Albania, "the post-war regimes came to reflect the demands of the Resistance Movements and their results" (pp. 496-97). Between the introductory "survey" and the brief conclusion lie four massive sections that deal with civil disobedience, intelligence, propaganda (the underground press figures heavily in this section), and military action.

Hæstrup has chosen a topical and analytical approach that allows him both to focus on specific features of Resistance as it manifested itself in various countries and to step back from time to time in order to view the European Resistance as a whole, as an integral part of the total history of the Second

World War. He concentrates mainly on problems of organization, logistics, and communications, which he elucidates with detailed information and statistics not easily found elsewhere. Where the book is deficient is in its failure to examine in depth the clash of ideologies and the often highly differentiated programs for liberation that animated the forces of Resistance. The author does make clear how the Allied governments utilized the Resistance movements, and here and there he alludes to the political goals of a number of these movements. Yet I think it is fair to say that for readers who are looking for an analysis of European Resistance from within, from the point of view of those who, in daily struggle, developed a sharply defined ideological as well as military conception of the significance of World War II, this book will prove to be rather limited and disappointing.

One of the great merits of Hæstrup's work is the exceptionally abundant information he makes available on the Resistance in Eastern Europe, especially Poland and the Soviet Union. He offers a perceptive analysis of the conflicts that divided Communist and anti-Communist Resistance forces in Poland and Yugoslavia and is very informative in his account of how the Russian Central Staff of the Partisan Movement organized and coordinated the vast irregular army of saboteurs and partisans in the occupied areas of the USSR. Despite the debilitating and isolating effects of the Nazi-Soviet pact of 1939, the author believes that "the Communist ideology, supported by experience, was prepared to resist the aggression of the Axis powers" better than other political formations (p. 468) and that, after June 1941, it was in the Soviet Union that partisan warfare "was best co-ordinated with the operations of the regular armies" (p. 486). He attributes this success, ironically, to the dictatorial character of the Stalinist regime and to the discipline imposed by an authoritarian one-party system.

The footnotes and the bibliography suggest that whereas Hæstrup has studied primary documents in Scandinavia, Poland, and the Soviet Union, he has relied entirely on secondary works (and not always the best ones) for his account of Resistance in Central and Southern Europe. Since he includes the Italian Resistance movement, it is disconcerting to discover that the names of Roberto Battaglia, Giorgio Bocca, Carlo Ludovico Ragghianti, and others are nowhere mentioned. The names of American historians are notably absent from the bibliography; Robert Paxton is the sole exception. Although several French historians are mentioned in the notes, Henri Michel appears virtually in godlike isolation in the text itself.

Let it be said, however, that despite its deficiencies and imbalances, *Europe Ablaze* is well worth



careful study by scholars and general readers interested in deepening their understanding of Resistance during World War II. The chapters on Eastern Europe are especially commendable for their wealth of information and judicious interpretations of all aspects of Resistance activity.

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MARIANNA P. SULLIVAN. *France's Vietnam Policy: A Study in French-American Relations*. (Contributions in Political Science, number 12.) Westport, Conn.: Greenwood Press. 1978. Pp. xv, 165. \$15.95.

De Gaulle's persistent and calculated resistance to American wishes in various areas of foreign policy not only irritated the state department but also elicited a rather substantial literature that might be described as *explications du général*. Marianna P. Sullivan's book falls within this genre. The subtitle reveals the author's subject: the role of Vietnam as an issue in Franco-American relations between 1963 and 1973.

The disagreement over Vietnam stemmed not only from de Gaulle's proverbial anti-Americanism but had its roots in three factors—relational, historical, and situational—that the author employs by way of analysis. These factors, which provide the book's structure, are self-evident. The relational factor refers to a set of issues that defined French relations with the United States, of which Vietnam was one. The historical factor traces long-standing differences over Vietnam arising from the Second World War, the first Indochina war of 1947–54, and the Geneva accords. The situational factor refers to alterations in French policy as the result of changing conditions in Vietnam during the high tide of American intervention. Although these categories enable the author to argue that French policy was more complex than simple anti-Americanism, they do not support many new or profound insights, much less any novel theoretical approach to international politics.

What the study provides is a brief but sometimes pedestrian account of the Vietnam issue as seen by de Gaulle and, to a lesser extent, by the state department. In the area of Franco-American relations, de Gaulle's criticism of American policy reflected his determination to obtain a more independent role for France on the international stage; Vietnam, like NATO, offered de Gaulle an opportunity to put some diplomatic distance between Paris and Washington. While crediting de Gaulle with a vigorous pursuit of French independence, the author disparages a "declining" power's quixotic effort to

take a global stance alongside the superpowers. In the realm of historical differences the author observes that the state department, obsessed with a struggle between communism and the free world, ignored de Gaulle's lesson that in an age of decolonization nationalism would prevail even over the power and the "pure" anti-imperialist intentions of the United States government. Instead, Washington dismissed the French critique as more galling anti-Americanism and so many sour grapes.

The author hesitates to draw judgments on the correctness of one side or the other, but she tends to question French motives. For example, de Gaulle's desire to end the war quickly before further escalation in the mid-1960s is seen as a tilt in favor of Hanoi that disqualified French claims to mediate the dispute. In retrospect we might give higher marks to de Gaulle's belief that an early settlement with Hanoi offered a greater chance for stability than a protracted war that could not be won. The author assesses the complexities of French policy toward Vietnam but inclines toward the view that it was ultimately self-serving.

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SIDNEY TARROW. *Between Center and Periphery: Grassroots Politicians in Italy and France*. New Haven: Yale University Press. 1977. Pp. xv, 272. \$15.00.

Comparative historians will find invaluable food for thought in Sidney Tarrow's insights, gleaned from an analysis of standardized interviews conducted with two hundred fifty mayors from four different regions in both France and Italy. It is by far the divergent political styles of the two nations, rather than the similarities in their local politics, that most strike the observer as a result of Tarrow's analysis. In both cases, to be sure, mayors function as policy brokers between the aims of highly centralized administrations and the competing demands of their local constituents, but the radically different administrative techniques in the two countries have elicited variant strategies of adaptation on the local level. The French administration is highly technocratic and coherent in policies that distinctly favor rapidly modernizing regions and that neglect declining areas. Mayors adapt to this system; they cultivate personal relationships with prefects and other bureaucrats and downplay political partisanship, claiming to be "apolitical." Their perceptions of their own achievements in office tend to reflect central administrative and bureaucratic values. As a corollary to this, central political choices are rarely challenged on the local level and democratic

dialogue is stifled; social conflict is rarely translated into politics, contributing to the blockage that erupts occasionally in upheavals such as May 1968.

In Italy, in contrast, central policy makers are paralyzed by internal ideological conflict, and the bureaucracy is inefficient, directionless, lethargic, and corrupt. Elites, to preserve their power, try to buy off protest with a coalition strategy and with distributive policies that favor declining regions and marginal groups at the expense of the dynamic sector of the economy and that create mass clientelism. Local life is highly politicized, for the mechanisms of both the Christian-Democratic and the Communist parties are essential conduits from the grass roots to the summit, means by which local politicians may bypass the inefficient bureaucracy to get things done. The costs of clientelism are legion: choice and dialogue are hampered, distribution of benefits is unpredictable and unjust, local energies are wasted on routine tasks, and the state loses legitimacy. In both Italy and France, Tarrow suggests, political integration and adaptation at the grass roots constitute a mechanism of exchange and consensus that thus far has helped the systems in both countries to survive; in neither case, however, are critical social issues addressed.

Tarrow's failings may be those of his discipline. He spends time abstracting models of the French and Italian experiences that may be quite inapplicable elsewhere, and he gives insufficient attention to crucial questions raised by differing national histories. Thus we are told that the Gaullist constitution is not the causal agent in the depoliticization of local government in France, but how local government actually functioned under the Third and Fourth Republics is never examined. What is meant by the assertion that the French Communist party, as opposed to the Italian, did not become a mass party "until recently"? One suspects also that several of Tarrow's tables are attempts to quantify the unquantifiable: years in office and electoral results are one thing, but "Level of Mayors' Party Experience by Degree of Early Family Politicization" may be quite another. These reservations noted, Tarrow deserves to be congratulated for illuminating a crucial aspect of the political experience in both Italy and France in a way that has theoretical implications for an understanding not only of local government but also of the broader political and social structures in both countries as well.

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ALAN MACFARLANE. *The Origins of English Individualism: The Family, Property, and Social Transition*. New York:

Cambridge University Press. 1978. Pp. xv, 216. Cloth \$19.95, paper \$6.95.

If the *AHR* illustrated its book reviews, *The Origins of English Individualism* could be nicely depicted with a drawing of Alan Macfarlane as England's patron saint, George, surrounded by the slain dragons of Marx and Weber, his peerless Smith-Corona having secured the castle of British historiography from foreign invaders. Without such a picture, a thousand words will have to be deployed to capture the excitement of this controversial and controverting book. Among its targets are no fewer than three dozen scholars, including an earlier Alan Macfarlane! Anyone interested in following what is surely going to be a major professional donnybrook should rush to buy a copy, but right away, for the uproar will not last long. The egregious errors Macfarlane has detected will be corrected. Acceptance of his major contention that England never had a peasantry, in the conceptual sense of the word, will come swiftly. Indeed, one can imagine dozens of blue pencils quietly excising the word "peasant" from manuscripts right now. Objections to his assertion that no fundamental transformation in the English economy took place between the thirteenth and the eighteenth centuries will also harden quickly. And, when these reactions have been registered, we shall understand a little better how a small country on the periphery of Mediterranean culture was able to move the center of the world to the fifty-first parallel.

Macfarlane's point of departure is the model of peasant society that anthropologists created in order to delineate the structural differences between traditional and modern societies. Drawn principally from studies of the Polish peasantry, this ideal type emphasizes the familial basis of property ownership, the geographic and social immobility of ordinary men and women, the absence of money and markets, and the dominance of patriarchs. Some people may feel that Macfarlane has written a lot of strawmen into his story, but, in fact, he does a fine job of protecting his flank by locating the elements of this model in current medieval scholarship. In four carefully crafted chapters he dismantles the edifice of English peasantry, leaving the interpretive scheme that relied upon it in shambles. For as far back as the records go English freeholders and copyholders, male and female, participated in a brisk land market. Not only were family members not tied to the land, they were not even tied to each other. Mothers and fathers disinherited their children; children resisted providing for aged parents. Nor do marriage patterns conform to the peasant norm. Extended families were rare, men and women married late, and sons and daughters did not supply the

labor for their own family's production. Nowhere in his examination of institutional arrangements from the thirteenth century onward does Macfarlane find the basis for community solidarity or an economy of household production and consumption. His message is clear: English society has always been different. Hence, no European-inspired model can aid in understanding the English.

Macfarlane has proven beyond the shadow of a doubt that Englishmen were not the Polish peasants of anthropological imagination; it remains for another scholar to do the same for the Poles. Because English folk were not peasants, however, does it necessarily follow that they were minicapitalists? Are there just two social types? For Macfarlane the answer appears to be yes: "The hypothesis . . . is that the majority of ordinary people in England from at least the thirteenth century were rampant individualists, highly mobile both geographically and socially, economically 'rational,' market-oriented and acquisitive, ego-centred in kinship and social life" (p. 163). What he has given us in fact are men and women marrying late, moving from place to place to work or wed, and possessing the right to alienate such property as might come their way. Macfarlane's response to these facts indicates the lingering influence of the original model, for it is only in contrast to the ideal peasant that such qualities suggest rampant individualism. No evidence is adduced to demonstrate that the thirteenth-century English were capitalistic in the sense of systematically investing in the productive process. It is also hard to know what he means by economically rational or market oriented. The men and women of rural England sold some portion of their harvest in local markets, but these terms normally referred to attitudes that promoted wider trade, regional specialization, and economies of scale. Price data show that England did not have an integrated national market until the seventeenth century. Nor is the possibility of alienating land proof of absolute property rights, for royal proclamations, statutes of the realm, customs of the manor, and municipal regulations hemmed in the use of personal resources in dozens of ways.

Macfarlane frequently summons to the bar of judgment a cluster of erring scholars. Thus, C. B. Macpherson and J. G. A. Pocock are arraigned together, as are Marx, Weber, and Thomas Babington Macaulay! What Macaulay shares with Marx and Weber is the conviction that at some time England experienced a period of decisive change, breaking not only with the rest of Europe but with its own past as well. For Marx the rupture came in the sixteenth century with the appearance of capitalistic modes of production. With Weber it took the rational principle carried by the Protestant rework-

ing of Christianity, while Macaulay looked to the time when God and Alexander Pope created the Newton of the Enlightenment. Macfarlane dismisses all three claims for change because the social habits of ordinary English men and women have not varied over time. Historians may well chafe at his procrustean conclusion. Not only is there a severe reduction of interpretive options, but there also lurks, unexorcized, the sociological assumption that the form of the society dictates events. Instead of seeing men and women selectively coping with concrete and changing situations, Macfarlane presents his English individuals frozen in a cultural holding pattern for six hundred years. Important as they are, Macfarlane's facts about mobility, marriage, kinship, and land sales cannot serve as surrogates for the dozens of other relevant variables. Indeed, the more he argues for the persistence of certain social arrangements, the less they can explain England's remarkable economic career.

To bring the matter down to something specific about English agriculture: the European-wide subsistence crisis at the end of the sixteenth century found London, alone, importing over one hundred thousand quarters of grain in a seven-month period. A hundred years later, when the Continent again had harvest failures, the English—now one-third more numerous—were actually exporting grain. Here is the evidence for a dramatic liberation from the age-old limits of growth. If Marx and Weber erred in associating a primitive agrarian technique with a peasant society, they were not wrong in thinking that something had happened to the old economic system and the political and intellectual order it supported. This new level of productivity reflected a major reorganization of the English economy. It impinged upon the relations between social groups and brought in its train a redirection of human effort toward innovation and efficiency. Alan Macfarlane has done a brilliant job of demonstrating why the English were ready for such a transformation. What remains to be learned is how his traditional individualism was made to accommodate the individualism of the modern, market society.

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JOYCE APPLEBY. *Economic Thought and Ideology in Seventeenth-Century England*. Princeton: Princeton University Press. 1978. Pp. x, 287. \$16.50.

There have been several good books on economic thought in seventeenth-century England—Max Beer's *Early British Economics* (1938), Sir Erich Roll's *History of Economic Thought* (1973), and William Let-

win's *The Origins of Scientific Economics* (1964). But they will all have to yield place to the book under review. Its novel features include a much wider and more detailed study of all seventeenth-century economic thinking, not just that of the great names. Joyce Oldham Appleby relates this thinking much more closely to economic life and action, starting in the 1620s when "the indisputable impact of declining cloth purchases, coin shortages, erratic exchange rates, and widespread unemployment forced contemporaries to focus on the purely economic factors disrupting the society" (p. 37). According to Appleby, the writings of Mun and Misselden, derived from practical considerations, marked "the beginning of an effort to isolate economic activity for purposes of analysis." "The challenge that the experts on trade laid down was not political but scientific. They did not debate the wisdom of sovereign power; they denied its efficacy" (pp. 47, 51).

Appleby frankly recognizes the ideological component of economic thought; this, indeed, is part of her thesis. The first chapter, "Models of Economic Development," states the theme: "To respond positively to the opportunities for further economic development meant to abandon customary ways of holding and working the land. It required the endorsement of new values, the acknowledgement of new occupations and the reassessment of the obligations of the individual to the society. Before these responses could be made, however, people had to perceive the changes and incorporate them into an intelligible account of their meaning. Before there could be new modes of behavior, there had to be ideas to explain them. . . . In the effort to understand and control the new commercial system, the writers created interpretive models that shaped the consciousness of their contemporaries and proved decisive for all subsequent thinking about economic relations" (pp. 4-5). "The historian who sets out to explore ideology is saying that the past cannot be understood without reconstructing the system of ideas through which the consciousness of men and women was organized. Implicit in this approach is the assertion that people do not choose their ideas freely but rather share in a socially constructed reality" (p. 278).

Appleby dates the origins of modern English social thought from the seventeenth century and sees an unmistakable break with the past during the English Revolution. With the collapse of personal monarchy, "power which men once justified through myth and ceremony" henceforth had to be explained and legitimated (p. 24). The practical economic treatises of the revolutionary decades gave birth to "the idea of economic laws and the conviction that anarchy was not the inevitable alternative to external control." Uniformities began to be assumed (p. 80).

Appleby attributes to increasing abundance the replacement of morality by science (pp. 57-59, 72, 80-85). As food manifestly became a commodity, to be bought and sold like anything else, an impersonal market with its own laws could be envisaged. The end of scarcity coincided with a freer economic life (pp. 87, 98). "The extension of market analysis from commodities to people and land was undertaken as an intellectual problem rather than as a moral decision." But it ended moral objections to enclosure and middlemen (pp. 84, 116-117). Most important for England was the Dutch model of a capitalist economy. The Dutch success story showed "the possibility of human ingenuity triumphing over the limits nature seemed to have set upon productivity." It was regarded with "a curious mixture of jealousy and admiration" (pp. 74, 78).

If market relations can be treated as predictable, human behavior must be conceived as sufficiently consistent to support that predictability. Trade becomes an inexorable process beyond the reach of human interference. Economic man is born: the personal drive for private gain becomes a legitimate and ineradicable human quality. What begins as an explanation of market practices becomes a justification (pp. 93-97). Individuals can be relied on to maximize their own profit. The market replaces the state as the regulator of human activity. Traditional concepts like "reason" and "nature" are transvalued in the process. Appleby illustrates this change by quoting two clergymen who argued for and against enclosure in the 1650s: "The nature and reason that Moore invoked to justify the magistrate's God-given power to control the economy for the good of the whole had become for Lee the mediator between self-interest and information from the market" (pp. 62-63).

Since English capitalism was mainly rural, the market invaded the countryside too. Appleby shows how the abolition of feudal tenures (1645; confirmed, 1660) "meant that decisions affecting the renting and working of land were made by private individuals in response to their own goals rather than by the king through policies for the public. . . . Land and labor. . . were brought into the commercial system" (p. 245). The late seventeenth century saw endless discussions of how to fit the reluctant poor for their new role as producers of wealth for the national and export market (pp. 144-57).

Among the many changes of the revolutionary decades, the Navigation Act of 1651 (confirmed, 1660) "represented a genuine departure in public policy, . . . placing the power of the state behind national economic development" (pp. 103, 111). Early seventeenth-century economists minimized the role of the state; but, of course, it all depended on whose state. With government in safer hands after 1688, "Locke's economic reasoning strengthened a new



sovereign . . . who . . . replaced the invisible hand of the market with the official hand of mercantilist regulation" (p. 271).

Appleby's analysis opens up large vistas. She attributes to late seventeenth-century economists "the notion that the wants of mankind were limitless"; Bernard Mandeville was an end rather than a beginning (pp. 182–84). We recall Hobbes—and Marlowe. Harrington's political theory fits very well into the assumption that economic behavior is predictable and therefore subject to scientific analysis. Bunyan's Mr. Badman illustrates the clash between old morality and new science. Appleby ends by suggesting that the clarity that the economists achieved in discussing coin and the source of market value in the 1690s "advertised the dangers of their intellectual freedom." Most thinkers recoiled to a balance of trade theory that preserved economic laws without sacrificing political direction in the interests of those who controlled the state (pp. 277–79). This is a stimulating, provocative, trail-blazing book.

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ROBERT ECCLESHALL. *Order and Reason in Politics: Theories of Absolute and Limited Monarchy in Early Modern England*. (University of Hull Publications.) New York: Oxford University Press, for University of Hull. 1978. Pp. 197. \$15.50.

This book is a revised doctoral essay. Robert Eccleshall begins by flaying his predecessors in the field, gives the customary thwacks to Christopher Hill for playing the game of "button, button, where is the bourgeoisie," and takes a pot shot at "Whig historiography." Having established that a new interpretation is necessary, he then gives us a brief outline of the major patterns of medieval political thought and gradually develops the thesis of his book.

The thesis is that medieval and Tudor political theory did not set royal absolutism in opposition to limited monarchy. Each was merely a variant of a more general theory in which government was regarded as divinely ordained, hierarchical, rational, and limited by the law of nature. In some kingdoms the monarch ruled alone; in others (limited or mixed) he was aided in applying the principles of natural justice by groups of wise men—parliaments, judges interpreting customary law, and so on. These groups were never seen as the spokesmen for individual interests: their function was to help the king determine the common good in accordance with the law of nature. In neither the limited monarchies nor in those in which the king ruled alone was there a sovereign. Eccleshall speaks of governments that were infallible—they always followed nature and

reason—but that were never sovereign—their unfettered will was not law.

Historians, Eccleshall argues, who seek the roots of modern theories of individualism and sovereignty in the writings of medieval and Tudor writers are guilty of reading the future into the past and thus distorting both past and future. A central chapter on Richard Hooker describes him as a brilliant theorist who summed up the medieval tradition and who has been consistently misinterpreted by writers who have sought to find in him modern conceptions of government. In fact, Hooker made no distinction between limited and absolute governments, had no notion of sovereignty, and did not believe that governments derived their power from the consent of a sovereign people endowed with individual natural rights.

The modern ideas of individualism, sovereignty, consent, and contract came into being suddenly after 1640. "Overnight, as it were, we move dramatically and irreversibly into a new world" (p. 153). Eccleshall gives a short sketch of why this happened, referring to a conjunction of social, economic, and political circumstances and citing the works of C. B. McPherson, Quentin Skinner, and Lawrence Stone.

The question raised by Eccleshall's thesis comes from this notion of sudden and catastrophic change from communalism to individualism. He reproaches earlier scholars for seeking the roots of the future in the past. But he appears to have adopted the paradoxical notion that the future has no roots and is, consequently, inexplicable in historical terms.

The book ends with a stirring epilogue in praise of Gerrard Winstanley that, by implication, condemns the individualism that replaced the communitarian thought of the Middle Ages and Richard Hooker.

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C. E. CHALLIS. *The Tudor Coinage*. New York: Barnes and Noble. 1978. Pp. xii, 348. \$27.50.

Some books have an importance beyond their ostensible subject matter. The present work is a case in point. C. E. Challis set out to write a history of the Tudor coinage and thus to fill a gap in our knowledge of how coins were made, by whom, where, under what influences from the side of politics and economics, and even the personnel assigned to minting tasks. He also intended to set out accurately changes over time in the size and composition of the currency, whether these were inspired by government (depreciations and debasements) or took



the form of criminal adulterations (sweating, clipping, and counterfeiting).

Readers of the author's many works on every aspect of the coinage will find here ample confirmation of the judgment that Challis is the master in the field. His new book again demonstrates that he is no mere expert in technical matters but that he has a wide-ranging understanding of administration, finances, fiscal policy, England's situation in the international economy, and the impact of political and military crises on England's circulating medium. His account of mining, mintage, bullion supplies, and the chronology of production sets firmly in place a coherent history of the five phases of Tudor coinage: 1485–1526, 1526–44, 1544–51, 1551–60, and 1560–1603. This enables us to understand the "great debasement" (1544–51) in context and also to estimate more properly the importance of the Elizabethan restoration of the integrity of the currency. For example, we should admire Wolsey's skillful settlement of the organization of the mint, which left little for Cromwell to do but to continue the reforms his master had begun. Cromwell, unfortunately, did not follow Wolsey in everything. Where the cardinal had brought the coinage to a peak of fineness, Cromwell experimented in debasement to pay the costs of fighting the Geraldine Rebellion in Ireland in 1534–35. Thomas Wriothesley, who had been Cromwell's chief-of-staff, continued these experiments and then extended them on a massive scale, with the help of other Cromwellians, to pay a part of the costs of the wars with Scotland and France after 1543. So began the "great debasement" that reduced the value of the currency to Englishmen at home and brought on a crisis in the European trade marts in what Challis calls a "fraud" and an unnecessary action by a government that failed to perform its minimum obligations to the community of the realm.

Many of the most important findings of Challis challenge current opinion about the revolutionary changes under Henry VIII that we associate with the work of G. R. Elton. Space does not allow a full setting forth of much interesting evidence. Hence I must merely mention a few of the choicest bits. Challis *proves* conclusively the existence of an imperial theory of the English crown before the advent of Cromwell (pp. 47–51). He shows, the revolution in government notwithstanding, that the "sovereign" state in the 1530s and after was unable to prevent the circulation of foreign currency in England and often gave it legitimacy by proclamation and statute. Moreover, Challis shows how private moneyers and those employed by Bristol, Norwich, and other borough corporations were able to issue coins of their own design and force the government to accept such infringements of the crown's established rights and prerogatives.

For anybody with an interest in the current state of Tudor studies, however slight may be their interest in coinage, this is a well-done and important book.

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EDWARD J. BRISTOW. *Vice and Vigilance: Purity Movements in Britain since 1700*. Totowa, N.J.: Rowman and Littlefield. 1978. Pp. x, 274. \$22.50.

*Vice and Vigilance* is a history of the champions of morality in Britain from the late seventeenth century to the present. Edward J. Bristow, assistant professor of British studies at New England College, Sussex, England, presents the reader with a carefully researched and innovative as well as intelligent survey of the many crusades fought in the name of morality. Bristow sees the history of vice and vigilance peaking four times during this period; thus the organization of the book. The first "realisation that policing vice was a social necessity" was in the 1690s, which witnessed the good intentions but futile results of the Societies for the Reformation of Manners. The next wave of moral outrageousness broke in the late eighteenth and early nineteenth centuries. This period of reform gave us the wonderfully righteous William Wilberforce. The 1880s marked a high tide of moral indignation with the organization of the National Vigilance Association (which lasted into the 1950s). Thanks to the diligence of the NVA British society was made safe from the sordid works of men like Zola, Maupassant, and Paul Bourget (for a while at least). The last peak of moral frenzy chronicled here erupted in the early years of the twentieth century. At this time the Public Morality Council was set up to strengthen further the policing efforts of the moralists. One of the successes of the PMC was the suppression of Radcliffe Hall's *Well of Loneliness* (again, only for a while). In an epilogue Bristow considers the more recent moral outcries plaguing British society as they are represented by the visions of Mary Whitehouse and Lord Langford; he shows us that morality is still a highly explosive issue. Bristow's purpose in writing this history was to demonstrate the impact that purity workers have had over the centuries in terms of initiating legislation, dictating standards for sexual expression in the arts and entertainment, and defining the nature of private attitudes to sexuality. At the same time, Bristow makes an interesting observation—these outbreaks of moral righteousness coincide with religious revivalism.

Bristow is a sensitive scholar. He demonstrates in a very cautious manner the complexities of morality

as it has been defined and forced upon British society. Although all the causes were put forth in the name of purity—to protect the vulnerable in society, not surprisingly defined in the majority of crusades as women and children—righteousness was not always right.

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MICHAEL IGNATIEFF. *A Just Measure of Pain: The Penitentiary in the Industrial Revolution, 1750–1850*. New York: Pantheon Books. 1978. Pp. xiii, 257. \$12.95.

North American historians seem to thrive on imprisonment. David Rothman gave us an intriguing book on America's discovery of the asylum in the Jacksonian era. Now Michael Ignatieff has fashioned from his Harvard dissertation an equally stimulating study of the emergence between 1775 and 1850 of the penitentiary as the dominant form of punishment in England.

Although Ignatieff's is the more narrowly focused work—indeed, it is in every sense more precise than Rothman's sometimes wordy excursion—the two studies share more than common chronology. Both claim to be “social” histories, a fashionable genre that suggests, misleadingly, that writing “institutional” history is in some way culpable. Neither study deals adequately with the earlier history of incarceration, a particularly lamentable omission in Ignatieff's case since we now have nothing on this topic between the end of Pugh's account in the fifteenth century and the beginning of Ignatieff's in the eighteenth. Common characters and institutions abound: Tocqueville and Beaumont, who toured America's prisons in 1835 (Rothman has 1831) and noted the irony that democracy is frequently characterized by intolerance toward “deviant” minorities; William Crawford, who was sent by the British government in 1834 (Rothman has 1832) to investigate the competing American systems and took great pains to prove that penitential discipline was a British invention; and, of course, the prisons themselves—Auburn, Sing Sing, Philadelphia—upon which the English drew heavily for both the design and regimen of their new institutions. At this point one realizes that the penitentiary movement and the materialist philosophy from which it evolved transcended national boundaries. An entire generation debated the origins of crime and argued the merits of congregate and separate systems, and of silence and the treadmill for disciplining and reforming the criminally inclined. The penitentiary, as both authors make clear, became a laboratory, a “grand theatre,” within which religious ideology and “scientific” experiments in hygiene, education,

and physical and moral reform were harnessed uneasily in a single existence.

Inevitably, however, a work on the English penitentiary is dominated by the austere vision of John Howard. “By 1860, most prisoners in Europe and North America marched to Howard's cadence and endured the solitude of his visions” (p. 209). The central concern of Ignatieff's study is to chart and explain the process by which the old public rituals of punishment at gallows, pillory, and stocks—all directed at the body—were replaced by Howard's solution of anonymous imprisonment—directed at the mind. On one level, it is a predictably grim tale of carefully regulated inhumanity. Millbank penitentiary in the 1820s and Pentonville in the 1840s stand comparison with any modern totalitarian institution; and, then as now, silence and solitude often ended in madness or suicide. To all of this Ignatieff brings an admirable restraint and a real gift for well-turned and telling description. On the other hand, the rapidity with which the “perfect” penitentiary was re-evaluated and modified in practice leaves room for optimism. Natural decline, disappointed ideals, and piecemeal philanthropy dominated this process, and it is misleading to claim, as Ignatieff does, that reforms resulted from organized political opposition. Here, and elsewhere, there is room for further investigation. Ignatieff's brilliant study will light the way for the detailed, and perhaps more pedestrian, work required to explain fully this complex and important stage in penal evolution.

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DESMOND KING-HELE. *Doctor of Revolution: The Life and Genius of Erasmus Darwin*. London: Faber and Faber; distributed by Merrimack Book Service, Salem, N.H. 1977. Pp. 361. \$12.95.

Erasmus Darwin (1731–1802) has often been praised as a physician, poet, Lunar Society member, and speculative evolutionary theorist—precursor to his famous nephew's more carefully supported work, *Origin of Species*. The present work, based on intensive study of Darwin's commonplace book and correspondence, as well as of his published writings, illuminates several additional facets of his work: especially his substantial mechanical inventiveness, his clever geological and meteorological insights, his thorough and thoughtful treatment of agricultural chemistry, and his impressive ability to design and execute experimental tests of his theories.

The many insights and bits of information to be

gleaned here are almost lost, unfortunately, in a maze of ahistorical judgments presented in cliché-ridden prose: Darwin was "at the height of his period of invention" in 1763 (p. 53), Franklin was the "Voice of America" in Britain (p. 61), Darwin's analyses of classes of bodily actions is "clear as mud" (p. 238), but above all, Darwin was "well ahead of his time" and his inventions were "a century or two ahead of their time." Virtually all of Darwin's ideas and devices are analyzed solely in relationship to how they fail or succeed in accordance with twentieth-century scientific knowledge. Thus, he is to be condemned because "he failed to anticipate Pasteur by adopting the germ theory of disease" (p. 241), and he is to be praised because he anticipated the doctrine of evolution in biology (p. 237), electrochemical theory (p. 296), the correct composition of water (p. 142), the theory of fronts in meteorology (p. 119), the theory of artesian wells in geology (p. 159), and the big bang theory in cosmology (p. 214), among others. Lamentably, most of these "anticipations" depend upon superficial considerations of modern ideas and generous or problematic interpretations of Darwin's words—words that are worthy of more careful study as intelligent and often spectacular developments of eighteenth-century ideas.

This present-mindedness of the author leads to a grotesque and unforgivable impatience with some of Darwin's physiological reasoning because it does not make sense in modern terms. Of Darwin's concern with the very foundations of human behaviors—their relation to the irritability of tissues, to sensation, to volitions, and to association—Desmond King-Hele tells us that "this phrase, ['directed by irritations, sensations, volitions, and associations'] is repeated *ad nauseam* in *Zoonamia* and really doesn't mean anything. . . . We can best avoid irritation by ignoring the phrase" (p. 245).

In sum, though there really is valuable information in this book, I would recommend keeping it out of the hands of those who are likely to be unaware of its extreme biases.

RICHARD OLSON  
Harvey Mudd College

JACK LIVELY and JOHN REES, editors. *Utilitarian Logic and Politics: James Mill's 'Essay on Government,' Macaulay's 'Critique,' and the Ensuing Debate*. Oxford: Clarendon Press. 1978. Pp. 270. \$17.95.

The editors have had the interesting idea of bringing together the whole of the argument between James Mill and the *Westminster Review* on the one hand and Macaulay and the *Edinburgh Review* on

the other over the question of Utilitarian theory, especially as it applied to political measures. Macaulay precipitated the argument on the eve of the public struggle over parliamentary reform. The argument generated great notoriety at the time; it continued over a year in the quarterlies, and it severely fluttered the doves of the Utilitarians. It also had the notable consequence of persuading John Stuart Mill that the premises of his father's political theory were "too narrow." The debate, as the editors call it, is worth reconstructing, for it brought into focus many of the key attitudes and issues in early Victorian thought about practical and theoretical politics; it also gave Macaulay an opportunity for one of his most slashing triumphs over opponents whose grave assurance of possessing the truth only made them the more vulnerable.

*Utilitarian Logic and Politics* includes the text that was the center of the debate, James Mill's "Essay on Government," originally written for the *Encyclopaedia Britannica* in 1820 and adopted by the young Utilitarians as "a masterpiece of political wisdom," in John Stuart Mill's words. Macaulay's three articles on Mill in the *Edinburgh Review* and the three replies appearing in the *Westminster Review* (the editors do not make clear that all three of these were the work of the *Westminster's* editor, T. Perronet Thompson) are reprinted; there is also an excerpt from J. S. Mill's *Logic* bearing on the problems of method raised by the debate. The editors add an appendix on "Utilitarian Ethics"; their major contribution, however, is a fifty-page introduction that concentrates on the two main terms of the title: the logic of the Utilitarian method and the politics of the Utilitarian program.

The introduction is remarkable for its thoroughness and closeness of analysis in seeking to establish just what the disputants meant by the language they used. Since Mill based his principles of government on the "laws" of human nature and since Macaulay asserted that these "laws" were utterly without scientific validity, the editors painstakingly examine what both sides might have meant or probably meant, concluding that Mill in fact promised more than he could deliver. On the political side, the editors provide a very clear set of distinctions among Whig, Tory, and Utilitarian notions of representative government and of their expectations about it. Though conceding the strength of Macaulay's offensive position, the editors show a sympathetic understanding of the defenses and purposes of the other side. For anyone who wants an informed and thorough account of the issues in this episode of politics and theory, here is the place to go.

THOMAS PINNEY  
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MARGARET PELLING. *Cholera, Fever, and English Medicine, 1825-1865*. (Oxford Historical Monographs.) New York: Oxford University Press. 1978. Pp. 342. \$18.50.

Margaret Pelling tells us in the preface of this book that it originated as an Oxford B. Litt. thesis, entitled "Some Approaches to Nineteenth Century Epidemiology, with Particular Reference to John Snow and William Budd." Little has been changed for the purposes of publication, and the book is a highly scholarly but sadly typical example of much academic publishing. It addresses itself to no major hypothesis or argument and consists almost entirely of a detailed description of mid-nineteenth-century medical notions and ideas, without any informing structure of analysis. It is only at the end of the book, in a newly written conclusion, that Pelling comes near to an interpretative statement that engages the reader's interest. Here she sets up for critical examination Ackerknecht's argument that the conflict between contagionist and anticontagionist theories of infection was rooted in economic, social, and political factors. She concludes (probably rightly) that the evidence does not support this kind of sociological analysis.

In one sense, the book is a reflection of the confusion that reigned in the minds of contemporaries about the explanation of disease. Although people like Snow and Budd were groping toward an understanding of the nature and origins of diseases like cholera and typhoid, a clear understanding of the process of infection had to wait, of course, until the development of scientific bacteriology at the end of the nineteenth century. Even on this, Pelling appears confused—she seems to be arguing that the earlier anticontagionist biological and chemical theories were equally as valid as the later bacteriological ones. Perhaps here is an example of the intellectual havoc and confusion that Kuhn and his followers have sown through their contradictory arguments on the nature of scientific explanation.

The book also suffers from being almost entirely an intellectual history of concepts of disease, with little grasp of the actual effect of illness on health and mortality patterns. As a result, Pelling can come out with highly inaccurate statements—for example, that smallpox did not decline "drastically through the eighteenth and nineteenth centuries." A cursory familiarity with the civil registration and case-fatality statistics of the period would have soon corrected this erroneous notion. Perhaps the over-concentration on the intellectual history of medicine is part of a reaction to the study of actual historical events, particularly in the area of disease and illness; but, clearly, in the long run, we will only understand the history of science and medicine when

we have understood the context of behavior in which they developed and operated.

PETER RAZZELL  
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SUSAN S. TAMKE. *Make a Joyful Noise Unto the Lord: Hymns as a Reflection of Victorian Social Attitudes*. Athens: Ohio University Press. 1978. Pp. 209. Cloth \$12.00, paper \$5.00.

This book claims to be about hymns and popular culture in nineteenth-century England, but it is really about the social message Susan S. Tamke finds in the wide range of texts she has studied. Thus, after a brief chapter on the history of hymns, there comes a discussion of Evangelicalism. Tamke argues, following E. P. Thompson, that Methodist hymns taught political quiescence and sexual sublimation. Similarly, the later nineteenth-century hymn was used by the dominant classes to inculcate morality and that ineffable Victorian quality of respectability.

Hymns for children proclaimed a dual, contradictory, and anxiety-producing message. On the one hand, the young were taught to be quiet and submissive and, on the other, to work actively for others: for family, society, God, and empire. The churches themselves, Tamke argues, ignored the social question as long as possible and responded slowly to the demand of the poor for social services and amelioration of squalid conditions. Hymns mirrored this not-so-benign neglect and the subsequent activism. Hymns, moreover, reflected the fortunes of empire; they proclaimed the civilizing mission of the English missionary to the benighted heathen and then, with a purported "faltering confidence" by the late nineteenth century, extolled instead world peace and brotherhood. In her final chapter Tamke analyzes hymn imagery and finds it redolent with an idealized past, military glory, family, serene countryside, and other symbols of conservatism.

As Tamke admits, "few ideas in this study are new." She does indeed dust off hundreds of old hymnals and bring their contents, albeit selectively, to light. But unfortunately she contributes little to our understanding of popular culture because she divorces the hymns almost entirely from the men, women, and children who sang them. None but commentators and pundits find voice in this study.

Whatever Tamke and modern scholars might think hymns reflect, we really need to know how people used and sang them. Given the drab and repressive message she finds so prevalent, one cannot help but wonder how they ever came to have their strong emotional hold over so many English men



and women of every class. No doubt many hymns in the nineteenth century did teach quiescence and sublimation. But the same century saw the Chartists sing Wesley's "Peace *doubting heart!* my God's I am" at the great Peep Green meeting and the Lud-dite martyrs go to the scaffold singing "Behold the Saviour of mankind/Nailed to the shameful tree."

Thus, not words themselves but the infinitely subtle, complex, and paradoxical ways in which people used words should be the stuff of popular culture. Hymns as texts are of marginal use; they tell only what we already know about Victorian society. But a study of hymns as sung in church and chapel as well as in camp meetings or political rallies might well reveal the recalcitrant nuances of the past.

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JAMES BENTLEY. *Ritualism and Politics in Victorian Britain: The Attempt to Legislate for Belief*. (Oxford Theological Monographs.) New York: Oxford University Press. 1978. Pp. xii, 162. \$15.50.

James Bentley has written a concise description of the interaction of ritualism and ecclesiastical and secular politics from the mid-nineteenth century to 1906. He argues that, despite decades of sporadic, inconclusive persecution and, occasionally, prosecution, highlighted by the ineffective Public Worship Regulation Act of 1874 and the ill-conceived heresy trial of Bishop King of Lincoln in 1889, ritualism in the Church of England not only survived but prospered. By 1906 ritualist priests constituted a vigorous and expanding minority of the Anglican clergy. Their stubborn defense of once-contentious liturgical practices was partially vindicated in that year by the findings of the Royal Commission on Ecclesiastical Discipline that, "The law of public worship in the Church of England is too narrow for the religious life of the present generation." The liturgical revisions accepted by the church since then, Bentley concludes, owe much to the ritualists. Perhaps even more importantly, however, their struggle against powerful religious and political opponents, including the queen herself, significantly advanced the cause of individual and religious liberty in England.

If "The Attempt to Legislate for Belief," as Bentley subtitles his book, obviously failed, the passage of the Public Worship Regulation Act and its consequences in the last quarter of the nineteenth century illustrated the serious divisions within the church and the persistent confusion of religious with political questions. In examining those divi-

sions and analyzing the confusion, the author relies heavily upon the unpublished papers of Archbishop Tait, Disraeli, Gladstone, Salisbury, and, for the first time, Lord Cairns. A selective survey of the provincial press provides some additional perspective on the impact of ritualist confrontations on areas outside of London. Although Bentley clarifies to some extent the cautious and manipulative role played by Disraeli in maneuvering Archbishop Tait and his antiritualist allies, most of the narrative will be familiar to students of the period. The interaction of church and parliamentary politics, so central to Bentley's study, has been treated much more thoroughly in, for example, Peter Marsh's *The Victorian Church in Decline* (1969). Within the narrow constraints of his short, scholarly monograph, the author has, however, surveyed the major events, personalities, arguments, and political and ecclesiastical machinations surrounding the ritualist controversy. At the same time little attempt has been made to analyze the subject in terms of the rapidly changing social, economic, political, or, surprisingly, religious climate of the period.

Bentley nevertheless offers some perceptive insights and raises several important, if undeveloped, issues. He describes, for example, the links between ritualism and the older Oxford Movement, despite the efforts of some former Tractarians, such as Pusey, to dissociate themselves from their liturgically more radical successors. The varying impact of different ritualist practices on Evangelicals, Broad Churchmen, and, to a lesser extent, Nonconformists is also made clear. Nothing offended Protestant sensibilities more dramatically than the institution of auricular confession, which, as Bentley emphasizes, was condemned not only as an insidious manifestation of Catholic infiltration but also as a prurient violation of domestic sanctity, female modesty, and sexual privacy.

Unfortunately, such explanatory references to popular, contemporary values are not developed, though they might make the extraordinary concern about ritualism exhibited by the Victorians more comprehensible. Similarly, while he notes that in some respects ritualism "merely reflected the general ornateness and 'conspicuous waste' of Victorian middle-class life" (p. 25), Bentley does not pursue the point. Ritualist priests, he explains, were generally younger men of high rank and independent means, who were in a position to resist the pressures from bishops, politicians, and parishioners. But his social analysis stops at this point, and he briefly describes, instead, the careers and conflicts of such familiar figures as J. M. Neale, G. A. Denison, and A. H. Mackonochie as well as several other patron saints of the Anglo-Catholic cause. Whether or not they are typical of ritualist clergymen is unclear,



nor are we told how many of their ordained brethren shared their sentiments. As a result, it is never apparent from Bentley's account just how extensive the ritualist "threat" actually was. He is more comfortable describing their beliefs, practices, and stubborn, often deliberately provocative, determination to resist first the pressures and then the laws directed against them. Some capitulated, some resigned or were driven from their livings, a famous few were imprisoned; but, as Bentley suggests, most simply continued their offensive practices until their bishops, like the politicians and the public at large, grew tired of the controversy and resentful of the harassment and oppression resulting from the unpopular and ultimately counterproductive Public Worship Regulation Act.

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DOUGLAS A. LORIMER. *Colour, Class, and the Victorians: English Attitudes to the Negro in the Mid-Nineteenth Century*. New York: Holmes and Meier. 1978. Pp. 300. \$23.50.

Conventional wisdom, at least of the upper-class variety, tended to assume that Great Britain had escaped the taint of racial prejudice so evident in the history of the United States. The reaction toward colored immigrants to Britain after World War II dispelled any such illusion, and Douglas A. Lorimer, in his very useful book, helps us understand the history of British attitudes toward blacks in an earlier era—the mid-nineteenth century.

Lorimer commences his study with a discussion of the black experience in England in the years 1600–1900. He contends that, although discrimination was in many ways the order of the day in the seventeenth and eighteenth centuries, there is little evidence of blacks as a group being treated differentially. He points out that, in general, the treatment of individual blacks was often comparable to that of whites in similar situations. "In eighteenth-century England," Lorimer avers, "racially prejudiced individuals undoubtedly existed, but on the whole, no clear pattern of institutionalized or socially sanctioned discrimination was in evidence" (p. 31).

The advent of the antislavery movement brought blacks more prominently into the public eye. Although sympathy for the black man in Africa probably increased, the association of darkness of pigmentation with inferiority was a consequence in England itself. Lorimer, nevertheless, does not find color prejudice constituting a major factor in the social fabric of Britain in the first half of the nineteenth century.

If antiblack prejudice was not significantly present in the first part of the Victorian era, why did it arise in the latter part of the nineteenth century? Lorimer asserts that the explanation lies in the developing Victorian class structure: "Once the idea of a 'black gentleman' became a contradiction in terms, the way to racialism was open" (p. 68). Put in another way, Lorimer argues that racialism in its more strident form grew out of the identification of blacks with their slave past and of the association of this servile status with the English lower orders. This developing state of affairs combined with the teachings of Social Darwinism and the precepts of "scientific racism" espoused by early anthropologists to make a particularly noxious brew.

After the introductory chapter Lorimer deals with the concept of the black gentleman in mid-Victorian society; he also discusses mid-Victorian philanthropy and the popular stereotype of the Negro as well as the influence of the American Civil War on British opinion. Chapters on scientific racism and Governor Eyre and the black revolt in Jamaica are of particular value.

All in all, Lorimer has written a book of considerable value. How and why racial antipathies developed is a subject of vital concern to historians and social scientists alike. Despite a tendency toward repetition, an excessive dependence on biographical and anecdotal material, and in some cases a dearth of analysis, *Colour, Class, and the Victorians* is a fine piece of work that should become a standard part of the literature on nineteenth-century Britain in general and the roots of racialism in particular.

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PETER BAILEY. *Leisure and Class in Victorian England: Rational Recreation and the Contest for Control, 1830–1885*. (Studies in Social History.) Buffalo: University of Toronto Press. 1978. Pp. x, 260. \$12.50.

"Leisure," says Peter Bailey, "was one of the major frontiers of social change in the nineteenth century, and like most frontiers it was disputed territory" (p. 5). How could the masses of working men, with their potential for social and political disruption, be enticed to spend their free time in ways acceptable to the Victorian ethos? The first, and most high-minded, proposal was to encourage the workers to pursue knowledge at mechanics' institutes. But these early machines of "rational recreation" soon proved to offer an unacceptable halfpenny's worth of entertainment and an intolerable deal of dull instruction, and the search for the right formula sought other paths.

It is at this point (ca. 1830) that Bailey's study begins. The problem was complex and unprecedented, the intended beneficiaries unpredictable and often unamenable: "the creature of custom that was the homo ludens of traditional culture was not transformed overnight into the free agent of an atomistic modern world" (p. 4). The reformers, churchmen and laity alike, tried one idea after another. Bailey examines two hopeful movements in detail, the workingmen's clubs and organized sports (mainly cricket and football). The publicans, who wielded an influence at least equal to that of the reformers, opposed both insofar as their business was affected, although sometimes they formed an uneasy alliance with the reformers when it seemed in their interest to do so. But it was these same publicans who came up with what proved to be the most popular new form of mass amusement, the music hall, "a prototype modern entertainment industry... because of the thorough-going commercialisation which accompanied its growth and affected all facets of its operation" (p. 150). Bailey's is the best available treatment of the music hall in Victorian society.

Agreeably written and level-headed, distinguished by shrewd estimates of the "realities" of the situation, this monograph draws extensively from primary sources both national and local. It repeatedly uses the town of Bolton, Lancashire, as a case study of what happened when the would-be controllers of the workingman's leisure applied their hopeful instruments of wholesome time killing in particular situations, notably in the new industrialized towns.

This exemplary contribution to our knowledge of English social history deserves a more attractive format than the publishers have given it. The text page, representing *reduced* typescript, is an ominous sign of the new age of the economy book.

RICHARD D. ALTICK  
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PAUL FELIX THIEDE. *Chamberlain, Irland, und das Weltreich, 1880-95*. (Europäische Hochschulschriften. Series 3, number 62.) Bern: Peter Lang. 1977. Pp. 403.

Anyone planning a life of Joseph Chamberlain may breathe easy: this book does not pre-empt the enterprise. The remarkable late-Victorian politician remains without an adequate biography, for that by Garvin and Amery lacks detachment and historical perspective, while Peter Fraser's 1966 study neglects the man for the statesman. Paul Felix Thiede merely uses Chamberlain as a peg upon which to hang an involved account of British political ideas

and parties during an eventful fifteen-year period. Undoubtedly, these years, beginning with Chamberlain's entrance into Gladstone's Liberal ministry and ending with his appointment as colonial secretary by the Conservative Salisbury, are profoundly significant in the total career of an individual who played an important role in the shaping of social reform, domestic politics, and imperial policy. But the individual is lost in the "isms," and the volume becomes a mere fragmentary contribution to biographical and historical knowledge.

Thiede has written a book that specialists will consult and nobody will read—which is simply to say that it is typical of the work of contemporary academic historians. Originally a Heidelberg doctoral dissertation, it exhibits the virtues and vices of its species. Solid, weighty, based on extensive research in manuscript and printed sources, buttressed with statistical tables and documentary appendixes, it unquestionably amasses a good deal of useful information. At the same time, however, it is arid, prolix, and in places a mere compendium of evidence. The six chapters, ranging from "Die Radikalliberalen und das Irische Landproblem" to "Das Weltreich als Nation oder das Weltreich der Nationen: Die Entstehung eines Konflikts," seem more like independent essays than connected parts of a whole. The great undigested blocks of quotations from parliamentary debates, commission reports, and political tracts not only induce the soporific syndrome inseparable from the dissertation format but also, together with the constant interpolation of phrases like "Local Government" and "Home Rule" in the German text, create a kind of pidgin effect that adds to the tedium. Still, it is unfair to condemn Thiede for practicing what the profession preaches—an emphasis on science to the exclusion of art.

Thiede rightly points to Ireland as the connecting link in the process by which Chamberlain and his contemporaries arrived at their theories of empire. Such a connection was perfectly apparent to Victorian Englishmen (to say nothing of the American anti-imperialist who warned that Puerto Rico would become "our Ireland"). Ireland's role as England's first colony and its influence in shaping English perceptions of—and conduct toward—subject peoples have, however, been obscured by the events of the last half century, and it is good to have them re-emphasized. One is struck, nevertheless, by the remoteness of the author and of the men about whom he writes from the object of their concern. A certain English politician is described here by a colleague as knowing "no more of land tenures in Ireland than he knew of land tenures in the moon." The same statement might be made about English politicians in general and applied to Irish condi-

tions in general. It was this ignorance of the distinctive circumstances of life and thought in Ireland that frustrated every attempt, no matter how well intentioned, to solve the "Irish Question." If, after centuries of contact, statesmen persisted in regarding the Irishman as a somewhat retarded Englishman, it is scarcely surprising that they displayed ambiguity and insensitivity in dealing with the peoples of their *Weltreich*. Seeing Ireland through the eyes of Chamberlain and his ilk, Thiede merely duplicates their imperfect and largely irrelevant vision of John Bull's other island.

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BRIAN HARRISON. *Separate Spheres: The Opposition to Women's Suffrage in Britain*. New York: Holmes and Meier. 1978. Pp. 274. \$24.50.

Fields such as the history of women and labor history have more than their share of practitioners who write what is, in essence, the Whig history of our time. We are, to be sure, all feminists now, and it has been correspondingly easy to neglect the views of those who opposed women's suffrage and, in doing so, tried to stem the tide of progress; the antisuffragists have been dismissed as unimaginative and narrow-minded defenders of the status quo with neither time nor logic on their side, sexual Adullamites predestined to draw, and deservedly so, the laughter of history.

Brian Harrison's new book is, then, doubly welcome—not only because it has no peers but also because of the empathy with which its author has explored a cause that now appears to have been so hopelessly indefensible. (Who today would seriously argue against women having the vote?) That considerable opposition to the enfranchisement of women existed among both sexes has long been obvious, but the nature of that opposition has never before been subjected to extensive scrutiny. Harrison dissects with care the remarkably complex nature of the opposition to women's suffrage and goes on to explore that opposition's numerous ties to certain gentlemanly habitats that proved to be fertile breeding grounds for antisuffrage sentiment—many of the leading "antis" had been nurtured by the peculiarly male ethos of the world of public school, Oxbridge, gentleman's club, and empire-stewardship in which women played supporting roles at best. It was no accident that Cromer and Curzon, those two proconsuls par excellence, became the leaders of the National League for Opposing Woman Suffrage, which by 1914 had a (primarily female) membership considerably larger than the

membership of the by then fast fading Women's Social and Political Union (WSPU).

Regarding the WSPU, Harrison argues that the militants' tactical excesses gave much succor to their foes and that the suffragettes grossly exaggerated the benefits that the vote, once gained, would bring. Both theses are sound, though by deliberately restricting his scope to the overtly political aims of the suffragettes Harrison has ignored the not inconsiderable psychological and symbolic importance of the vote; Christabel Pankhurst herself wrote in 1913: "Another mistake that people make is to suppose that we want the vote only or chiefly because of its political value. We want it far more because of its symbolic value—the recognition of our human equality that it will involve."

As the first investigation of the opposition to women's suffrage in Britain, this well-written book is original and invaluable. Scholars concerned with the history of British women must seriously hope that Brian Harrison will, in due course, complete that "larger inquiry . . . into women's organisations in Britain between 1900 and 1940" to which he alludes in the preface to this work (p. 11).

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ZARA S. STEINER. *Britain and the Origins of the First World War*. (The Making of the Twentieth Century.) New York: St. Martin's Press. 1978. Pp. vi, 305. \$16.95.

These are intellectually exciting times for historians of diplomacy and international relations. In a process of self-transformation, which is both rich and complex, relationships are being explored not only with economic, social, intellectual, institutional, and psychological history but also with those social scientists of Karl Deutsch's third and fourth waves who, at all levels of analysis and across several issue areas, seek to explain the functioning of the international system and the behavior of its actors. Zara S. Steiner, in the brief introduction to her altogether admirable book, probes some of the problems and tentative results of the exploration of those relationships, and that in itself is a significant and welcome contribution.

She is particularly and justifiably concerned with what our brethren in the social sciences describe as the explanatory potency of the domestic variable and what they analyze under the term linkage politics. She investigates this central theme by testing hypotheses about the relationships between domestic factors and political instability on the one hand and foreign policy behavior on the other. She concludes unequivocally that external and diplomatic factors rather than the domestic situation largely

determined British foreign policy and ensured that Asquith's Liberal government would take the nation into war as a result of the escalating crisis in the summer of 1914. Fritz Fischer's thesis, therefore, in Steiner's opinion, does not adequately explain British behavior. In addition, she is concerned with what might be described, on the heels of Maurice Cowling, as a form of high politics and with the nature of the very firm control that certain individuals and a self-perpetuating elite were able to maintain over the conduct of British foreign policy. Again, the argument is skillful and convincing.

One is particularly impressed with the way that Steiner is able to introduce and manage several themes. Economic factors, party political considerations, societal forces, and the intellectual atmosphere are fused with the central consideration, foreign policy formulation and behavior. Macro-factors are related to individual and elite behavior, the European and imperial theaters are skillfully interwoven, and the author switches easily from the conflict and cooperation existing within each European capital to an analysis of the relations among the various capitals. With the sure touch of a scholar who has researched the period and mastered the research of others, as the excellent bibliography demonstrates, she traces the forces affecting the decisions and actions of the various British governments from the turn of the century to 1914. Students could not wish, therefore, for a surer guide to the diplomatic revolution that took place before 1906, to Britain's relations with Germany, France, and Russia, and, to a lesser extent, with the Habsburg monarchy, Italy, and the United States, and to the role of the bureaucracy and the defense establishment and their relationship to the cabinet. In particular, and as one would expect, Steiner has full command over the policies of Sir Edward Grey and his professional advisors and permanent officials. In fact, her analysis of Grey in many ways dominates the book and is quite authoritative. Her work, along with that in the volume edited by F. H. Hinsley and entitled *The Foreign Policy of Sir Edward Grey*, virtually exhausts the subject. The debates and arguments will no doubt continue, but the files of the Foreign Office have surely surrendered their worthwhile information.

In sum, Steiner's range is broad, her propositions are convincing, her analyses sure, and her judgments astute. Undergraduates may be somewhat confused by the failure to identify certain individuals. The chapter on the 1914 crisis, and this was true of her earlier work on the Foreign Office, is too brief and less rich than one would prefer. Perhaps she feels that there is little that is either new or significant to add, and many would agree with her. In any case, this volume is a decided success, a major

contribution, and a fine and judicious blend of the author's own research and the literature in the field.

MICHAEL G. FRY  
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WILFRIED FEST. *Peace or Partition: The Habsburg Monarchy and British Policy, 1914-1918*. New York: St. Martin's Press. 1978. Pp. xiv, 276. \$16.95.

One of the principal casualties of the First World War was the Habsburg Empire. Its destruction was not sought by either the British, with whom it traditionally enjoyed good relations or, for that matter, by the French. Britain and the Habsburgs had not been at war since 1763. The reluctance that existed in 1914 was summed up by an Austrian official: "It seems absurd for good friends like England and Austria to be at war."

However reluctant the British might have felt, the fact remained that they were at war with the Habsburgs, who were Germany's main ally. The stalemate on the western front, which necessitated defeating Germany by diplomatic means, determined their attitude toward Vienna. From 1914 to 1918 there were three distinct phases of British policy. The first was based on the premise that a separate peace was out of the question as long as Franz Joseph was alive. It lasted until his death in November 1916. During this period Britain sought to win new allies at the expense of the empire. It lured first Italy and then Rumania into the Allied camp with promises of support for the Habsburg territory each coveted. The second phase lasted from the accession of the new emperor, Karl, to March 1918. Aware of his interest in ending the war, Britain sought to detach Austria from Germany but encountered a major obstacle in its secret treaties with Italy and Rumania, which removed the actual possibility of a settlement the Habsburgs could have accepted. Nevertheless, London made three serious attempts to win over Vienna. On each occasion Karl's refusal to abandon the Germans saved Britain from having to reckon with the consequences of its duplicity.

The third and final phase reflected the desperation of the Allied cause. Until March 1918 British war aims had not included championing the right to self-determination of any of the principal ethnic groups within the empire. With the front now overrun the British reluctantly did so, thus abandoning the Habsburgs. To a certain extent recognition constituted a notable vindication of those in British public life who had championed the cause of the subject races since the beginning: Wickham Steed, R. W. Seton-Watson, and other influential supporters of the "New Europe."



Wilfried Fests book is a valuable contribution to the subject of Allied war aims. He has shown how hesitant the British were to destroy one of their principal adversaries and, until forced to do so, to embrace the weapon—so dangerous to their own empire—of self-determination. His work is based on all of the major extant archival sources. The book has few errors (Alex Leeper on page 226 should be Allen). There are only a few hints that English is not the author's native language. A minor criticism is that he possibly overemphasizes the importance of Crewe House and the Political Intelligence Department in winning the government's support in 1918 for an abandonment of the Habsburgs. Events on the western front forced it to move in that direction. An omission is the attitude of George V to the various phases through which British policy went. With so much interest focused on a fellow monarch, some information should have been included. All in all, Fest's first book holds the promise of much fine scholarship to come.

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University of Alabama,  
University

MARTIN PUGH. *Electoral Reform in War and Peace, 1906-18*. Boston: Routledge and Kegan Paul. 1978. Pp. xi, 228. \$16.25.

In 1914 about 60 percent of adult males, and no women, had the vote in Britain; by 1918 all men over twenty-one and women over thirty had it. This substantial step toward democracy happened so quietly that Martin Pugh has really done very well to make it so interesting (his book deserves something better than the photocopied typescript in which it has been published). The very visible efforts of the suffragettes hardly affected the situation: Pugh offers a penetrating study of the position in 1914, showing how poor the women's prospects were in the face of the opposition of the prime minister and the House of Lords, pouring cold water on McKibbin's recent argument about the Labour Party's chances of expanding its electoral base, and briefly explaining the system of registration for voting. It was the complexities of registration that deprived most of the unenfranchised males of the right to vote, and it was the problems of the electoral register rather than speechifying about democracy that precipitated wartime reform. A general election was due by December 1915 and, though it was put off, politicians could never ignore the possibility of an election and the need for an up-to-date electoral register. A register acceptable to the public could hardly disenfranchise men who had forfeited their 1914 qualification by joining the army or

moving to munitions work, and there was pressure to enfranchise all soldiers, whatever their position in 1914 had been. Thus, questions of electoral law could not be left until after the war. Late in his premiership Asquith set up a committee under Mr. Speaker Lowther to discuss them, and it worked out a businesslike compromise between the parties.

Once the question was raised, simplification of registration was inevitable, and this meant universal male suffrage. Votes for women had sunk almost out of sight during the war; Lowther, who had been condemned as anti-suffrage because of his rulings on the amendments to the 1912 electoral reform bill, nudged his committee forward to female enfranchisement at a time when obstructionism might possibly have prevailed: Pugh gives some substantial evidence for his argument that the majorities for enfranchisement were rather more fragile than they looked. Perhaps he allows himself a few speculative judgments in the later stages of the book; he suggests that the 1918 Representation of the People Act passed only because of some unlikely political circumstances, which undercuts the argument of the earlier chapters showing what drove Asquith to set up the Lowther committee. He also suggests that the Conservative dominance of the following twenty years might have been prevented by the proposals for proportional representation or for the alternative vote which survived until very late in the passage of the bill, which (apart from assuming that under such a system everybody would have voted as they did under the existing system) takes no account of the way these changes were proposed for some parts of the country and not for others. But perhaps these problems should be seen as spurs to further thought rather than weaknesses in a helpful and valuable book.

TREVOR LLOYD  
University of Toronto

P. B. M. BLAAS. *Continuity and Anachronism: Parliamentary and Constitutional Development in Whig Historiography and in the Anti-Whig Reaction between 1890 and 1930*. (International Archives of the History of Ideas, number 91.) Boston: Martinus Nijhoff. 1978. Pp. xvi, 441. f 90.

This is an ambitious book, one that aims both to study nineteenth-century Whig historiography and to show how, in the period after 1890, British historians reacted against its premises and conclusions. In addition, changing modes of historiographical practice are linked to social and political factors, as well as to developments within the discipline of history.

In his first chapter P. B. M. Blaas points out that



criticism of Whig historical writing, with its characteristics of "anachronism, finality, and historical continuity" (p. 3), antedated the work of Namier and the publication in 1931 of Herbert Butterfield's *The Whig Interpretation of History*. The stronghold of the Whig interpretation lay in constitutional history; hence, even before 1890, both the "imperial" history of Seeley and the emergence of economic history began to displace Whiggish constitutional history from its position of pre-eminence. Blaas also uses the Kuhnian approach in order to demonstrate that as historians such as J. H. Round and G. W. Prothero increasingly exploited the archival records, they were confronted by an "anomaly-experience"; that is, the ever-growing mass of data could not be accommodated within the Whig paradigm.

After having prepared the reader in his first chapter for a further account of the decline and fall of Whig historiography, Blaas shifts his focus in the second chapter to a lengthy discussion of nineteenth-century Whig historical writing. He examines the work of the early nineteenth-century medievalists F. Palgrave, J. Allen, and H. Hallam; of Macaulay; and finally, of the members of the "Oxford School": W. Stubbs, E. A. Freeman, and J. R. Green. His discussion centers on how these historians (and others) interpreted such crucial themes as the development of the medieval parliament, the Civil War and the Glorious Revolution, and the reign of George III. Without indulging in a ruthless reductionism, Blaas emphasizes the relationship between the treatment of these themes and the agitation surrounding the passage of the 1832 and 1867 Reform Bills. His conclusion is that the English political climate, particularly during "the age of equipoise," at once contributed to, and was the result of, the Whig glorification of the English tradition of constitutionalism and self-government.

The subsequent sections of this book are the most original and interesting. In them Blaas traces the process whereby Whig historiography was undermined by changes in political attitudes, as well as by the growth of professionalism among historians. The obstructionism of the Irish representatives in the House of Commons, the crisis in the House of Lords in the first decade of the twentieth century, the governmental incompetence revealed during the Boer War—all of these factors led to a questioning of the *reverentia antiquitatis* that had been central to the Whig view of English history. Blaas argues, following J. G. A. Pocock, that criticism of tradition may lead, and in this case did lead, to a breakthrough in the writing of history. It is in the work of F. W. Maitland that Blaas finds the first fruits of this breakthrough. He cites Maitland's "endless hunt for anachronistic representations of the past" (p. 266); his insistence that English medievalists dis-

card their insularity in favor of a comparative approach; his desire to broaden constitutional history into a study of the actual working of institutions. Substantively, Maitland's conclusions about the composition and functioning of the medieval parliament marked a break with the accepted Whig interpretation. The other two protagonists of the anti-Whig reaction were A. F. Pollard and T. F. Tout, according to Blaas. Pollard's studies of the Tudor monarchy and, especially, his *Evolution of Parliament* constituted a root-and-branch attack on traditional views of English history. An adherent of that set of ideas usually termed the *thèse royale*, he saw the power-seeking efforts of medieval and Lancastrian parliaments not as steps toward self-government but as impediments to the development of the modern state. Only after the establishment of order and the destruction of privileged bodies by the Tudor monarchy did self-government emerge as a possibility for the citizens of a unified national state. Pollard was intensely aware of "the irony and accident of historical development" (p. 300), and this sensitivity to the unintended consequences of historical action caused him to oppose the finalism of Whig historiography. Tout's work in administrative history simply by-passed the conventional concerns of nineteenth-century Whig medievalists: he was convinced that more could be learned about medieval English politics and society by meticulous study of administrative routines and practices than by concentrating upon parliaments, constitutionalism, and ideology. In developing this approach Tout was influenced by the French school of administrative and institutional historians (C. Langlois, A. Luchaire, and P. Viollet), all of whom contended that individuals and ideologies were of lesser importance than the slow, centuries-long growth of the administrative machinery of the state. By stressing the distinction between politics and administrative developments, Tout achieved a new and more objective treatment of medieval English history.

In the concluding pages of his book, Blaas returns to his central thesis: progress was made in England between 1890 and 1930 toward a more scholarly historiography that rejected the anachronistic and finalistic features of Whig historical writing. This new historiography was characterized by painstaking archival research, emphasis upon "facts," and specialization. Although he does not denigrate the achievements of this genre of historical writing, Blaas points out its defects, especially its inability to provide an integrated account of sets of historical phenomena. He finds in Norbert Elias's concept of "figuration" analysis an example of how it is possible to construct a frame of reference within which the interrelatedness of the most varied historical phenomena becomes apparent.

It is impossible in a short review to give an ade-

quate impression of the breadth and erudition of this book. The author's command of the sources necessary for explication of the historiographical views of twentieth-century British historians is particularly impressive, since these views are scattered in reviews and occasional pieces that sometimes appeared anonymously. It must be said, however, that the style and organization of the book leave much to be desired. Possibly the infelicities of style are the result of poor translation (the book was originally published in Dutch); in any case sentence after sentence evokes nothing so much as a caricature of nineteenth-century German academic prose. There is far too much detail on topics not central to the subject matter of the book, as well as a good deal of repetition. Despite these defects, Blaas has produced a thoughtful and important book, one that has the great merit of placing the work of late nineteenth- and early twentieth-century British historians within a larger social and historiographical context. His conclusions make it evident that the historians of the anti-Whig reaction were sophisticated practitioners of their craft, aware of the difficulties and pitfalls that beset historical writing.

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PAUL HAYES. *The Twentieth Century, 1880-1939*. (Modern British Foreign Policy.) New York: St. Martin's Press. 1978. Pp. viii, 343. \$15.95.

This account of British foreign policy centers on the reduction of Britain from a world to a European power. It has the initial merit of a conventional study of foreign policy. The approach lacks the wider focus of such authors as D. C. Watt and Max Beloff and the overstraining breadth of Maurice Cowling, authors who are not cited in the notes or in the author's bibliographic acknowledgment.

From a volume in "Modern British Foreign Policy" the reader might expect, and possibly fear, a textbook synthesis. It is not, but the work may have developed from middle-range Oxford lectures. Material is presented in densely argued chapter divisions with possibly too many theme punch lines to do the work of narrative organization. The author makes references, such as that to the British Agreement with China, June 9, 1898 (p. 90), without the identification that most readers need. Paul Hayes confronts the expectation of a textbook when he provides a prefatory description of his book "as a personal view of the events of 1880-1939" with all the imperfections and, he could add, the advantages of intense feeling (p. vii). The emotional participation is everywhere as Hayes attempts to provide a historical answer to "how and why did Britain's decline take place?"

The answer is complicated because Britain was not a power state, some of its weaknesses appeared in the decades of its apparent zenith, and, after World War I had inflicted irreparable damage, its position in the world was not effectively reduced or redefined. Indeed, Hayes emphasizes that until the 1960s Whitehall did not recognize or understand Britain's inability to pursue world policies.

The beginning, Gladstone and Granville (1880-1885), is in the minor mode. Gladstone's moralism (which Hayes formulates somewhat primitively) and its frustration left a baleful legacy, particularly in Egypt. Salisbury took over to restore the old diplomacy and Britain's credibility. Many well-selected quotations from the Conservative leader, whose papers Hayes used, present him as a trenchant realist. After an interval of Rosebery's difficulties with a divided party, and his own drive and abrasiveness, Salisbury returned to preside over Britain's diplomatic revolution, its departure from a nonentanglement policy to one of concentrating on areas where Britain could protect its interests. By 1914 "Grey and Bethmann Hollweg were both prisoners of the past, of the actions of others and even themselves" (p. 171). Grey figures among the disasters but insufficient attention is given to the case of Grey's defenders, for example, R. J. Sontag.

War brought conditions to which Britain failed to adapt. Contradictory conditions were met by contradictory policies compounded by unrealism. British leaders of the next decade (1923-33) receive understanding treatment. At its end the situation begins to be "Past Redress." The account of appeasement is written in anger. Hayes will have no revisionism in his critique of appeasement, but he justly refuses to make a hero of an enemy, such as Vansittart, of his enemy, Chamberlain. He is very good on the strange gagging drama of appeasement prolonged into wartime.

Hayes has written an uneven but very readable book on maladaptation and decline, a haunting subject today.

M. A. FITZSIMONS  
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MICHAEL J. COHEN. *Palestine, Retreat from the Mandate: The Making of British Policy, 1936-45*. New York: Holmes and Meier. 1978. Pp. xii, 239. \$23.00.

Michael J. Cohen gives us an in-depth study of British decisions and their sources vis-à-vis Palestine from the Great Arab Revolt until the end of the Second World War.

Professor Cohen's thesis is that British decisions relative to Palestine were primarily based on anticipated and actual reactions in the greater Arab world and later by its two wartime allies, the

United States and the Soviet Union, lest Britain lose its influence in the Middle East and be forced to surrender control of the Suez Canal. This is not surprising, yet we can be grateful to Cohen for his archival research in substantiating his arguments. Indeed, I believe he is the first author to publish or synthesize the recently opened P.R.O. documents on the Second World War as they relate to the Middle East. He has also made good use of the Central Zionist and Weizmann Archives. In addition to these sources, Cohen has corresponded with and used the comments of Sir George Rendel, the head of the Foreign Office's Eastern Department and, as such, the most important civil servant concerned with Palestine.

Despite its title, *Palestine is not the focal point* of this book; interdepartmental policy making is. Here the two major protagonists are the Foreign and Colonial Offices—the former generally on the side of the Arabs, the latter frequently supporting the Zionists, with the Foreign Office usually determining policy, even during the Prime Ministership of the pro-Zionist Winston Churchill. Unfortunately, because of Cohen's overriding concern with the decision-making process, events in Palestine itself are somewhat blurred. While at least three chapters are devoted to the Arab revolt of 1936-1939, nowhere are any statistics or analyses to be found which would illustrate the ferocity of the struggle: we are merely told that it had to be dealt with, but not what it was. The bureaucrats in London did not have to work in a vacuum; neither should Cohen's readers.

Two other problems mar the excellence of Cohen's book. The more important of these is his frequent refusal to maintain a chronology. All too often we are introduced to events before any preparatory material has been provided only to experience a sense of *déjà vu* when, within the same chapter, or perhaps a subsequent one, we find the background for which we had searched before. On occasion such techniques are necessary to continue the smooth flow of an argument, but Cohen resorts to this too often, the resultant effect being confusion rather than clarification. The second (and related) difficulty is with the notes. While they provide a valuable jumping-off point for other scholars to carry on Cohen's work, too often they are either irrelevant or belong in the text or seemingly (at least) they occasionally contradict a statement or opinion found in the text itself.

But these reservations do not take away from the central value of Cohen's book to the advanced student of the field. Cohen writes well, has worked hard, and the scholarly contribution he makes by his archival research is enormous.

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ROBERT R. HARDING. *Anatomy of a Power Elite: The Provincial Governors of Early Modern France*. New Haven: Yale University Press. 1978. Pp. x, 310. \$20.00.

Owing to the importance of provincial governors in the politics and society of early modern France, a number of historians have attempted to relate the evolution of the governors' power and prestige to such larger issues as the rise of royal absolutism and the monarchy's changing relations with the great aristocracy. But, because most research on this "power elite" has been narrowly focused on a particular individual or region during a limited number of years, these efforts have frequently resulted in broad generalizations that lack sufficient substantiation. By analyzing the careers and regimes of many governors over a long period, Robert R. Harding provides a perspective that only a general study affords. Basing his investigations primarily on a collective biography of the 142 governors appointed to France's eleven major *gouvernements* between 1515 and 1650, he offers an original and remarkably comprehensive view of the major transformations that swept provincial administration in this era.

Even when supplemented by other sources, the use of group biography entails an obvious risk: in this instance, governors were a highly individualistic breed, and not all of them conformed to the broad patterns the author discerns in their group experience. But it is precisely the synthetic quality of this book that constitutes its principal strength. So much is accomplished that one can easily regard the few unresolved issues and an occasional lack of elaboration of an important point as agendas for further research rather than as significant shortcomings.

The first and third sections of the book describe in two chronological steps the function and personnel of the governors' regimes and explain changes in them. The second section, which provides a quantitative analysis of information on the governors' wealth, offices, marriages, and families, places the political and administrative activities of the governors in the context of their changing social and financial circumstances and goals. Throughout, Harding draws on a variety of archival materials, although one misses a general discussion of sources in a book that will become a model for future research on the governors and perhaps on other elite groups as well.

Two broad themes emerge in this book. First, Harding traces the growing religious partisanship of the governors in the decades after 1560 and convincingly attributes this conduct to a collapse in the traditional basis of their provincial authority—patronage and brokerage. By assuming leadership roles in regional party organizations, the governors

prolonged the Wars of Religion, but they also found the means to reassert their local domination. Second, Harding persuasively challenges the widely held view that the fading significance of the governors in the first half of the seventeenth century resulted from conscious royal attempts to replace their "patrimonial" administration with the more bureaucratic system of intendants and commissars. Instead, the gradual withdrawal of the governors from regional affairs was largely a voluntary response to a wide range of long- and short-term processes that drew them to the court and the capital. Although the intendants slowly filled this vacuum, they did not create it.

This broadly conceived and challenging study merits a large audience among European historians.

ALBERT N. HAMSCHER  
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NICOLE LEMAITRE. *Un horizon bloqué: Ussel et la montagne limousine aux XVII<sup>e</sup> et XVIII<sup>e</sup> siècles*. Preface by PIERRE GOUBERT. (Mémoires et Documents sur le Bas-Limousin.) Ussel: Musée du Pays d'Ussel. 1978. Pp. 239. 75 fr.

In this work Nicole Lemaitre uses two words to describe eighteenth-century Ussel—stagnation and mediocrity. Stagnation is evident in the lack of demographic growth, in the depressed economy, and in the shocking 50 percent decrease in literacy from about 1690 to 1780 as measured by the ability of newly married men and women to sign their names. Mediocrity is apparent in the small dowries and sparse household inventories.

The century of Enlightenment and progress had obviously bypassed Ussel, the *chef-lieu* of a *sénéchaussée* in eastern Limousin whose population was approximately twenty-eight hundred at the end of the *ancien régime*. Lemaitre's work leads to the inescapable conclusion that the general demographic and economic advances of the eighteenth century were unknown in this remote area of France; the inexorable march of "progress" would have to wait for another place and time. In fact, it is shockingly clear that life in Ussel, whether measured demographically, economically, socially, or intellectually, was actually worse at the outbreak of the Revolution than it was a century before.

Lemaitre's work is an excellent example of what a local study should be but too often is not. It is well organized and researched, clearly written, has charts and tables that do not overwhelm the text, and includes very helpful and informative illustrations and pictures. Its organization is clear and simple. One section is devoted to the people of Ussel. In a brief preface, Pierre Goubert tells us to be properly impressed with the work that went into

Lemaitre's analysis of one hundred and fifty years of data from parish registers. The use she has made of these data is truly impressive. Of course, she had an excellent mentor in Goubert.

A second section discusses the activities, primarily economic, of the people who lived in Ussel. Finally, "Presentation d'une Société" focuses on the social and economic structure of this society and on the various groups—from nobles to sharecroppers—who inhabited it. Lemaitre gives the reader, both in words and pictures, a feel for the *pays* of Ussel, cut by numerous gorges, spread out over more than fifty hamlets. Her descriptions of a peasant's house, of the hamlets themselves, and of the ville—its walls, gates, *boutiques*, and *faubourgs*—are excellent.

Those of us who are interested in the history of preindustrial rural areas—wherever and whenever we may wander—would be well advised to appreciate what a fascinating, craftsmanlike job Lemaitre has done. Local history has certain advantages, both for the writer and for the reader. This is an excellent example of how it should—and can—be done.

THOMAS F. SHEPPARD  
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IRA O. WADE. *The Structure and Form of the French Enlightenment*. Volume 1, *Esprit Philosophique*; volume 2, *Esprit Révolutionnaire*. Princeton: Princeton University Press. 1977. Pp. xxiii, 690; x, 456. \$60.00 the set.

Ira Wade's basic quest, like that of many of his predecessors, is the unity of the Enlightenment. He finds it in the belief that what one does is the product of what one thinks. "There is an absolute connection between thinking, living, saying, and doing" (1: xxiii). Moreover, thought and action in any of the seven categories of life, which Wade defines as religion, politics, economics, morality, science, esthetics, and the life of the self, affect all of the others. The author analyzes all of the elements that went into the structuring of Enlightenment thought, the unified shape and character of its philosophy, and how the Enlightenment turned from reflective thinking to an implicit call for revolutionary change. The transition from a philosophical to a revolutionary mentality was the essence of the Enlightenment's unity because it expressed the belief that acting results from thinking and that civilization is created by ideas.

The first part of this massive study examines how the Enlightenment was formed by the intellectual developments of the previous two centuries. The "esprit philosophique," wherein philosophy became the study of science, morality, and knowledge through free, skeptical inquiry based on the accumulation of facts, was derived from such sources as



Horatian poetry, humanism, travel literature, new scientific knowledge, classical French literature, the free-thinkers, and systematic philosophy. The coalescence of the whole range of thought from Montaigne to Bayle in the mind of Voltaire and his conversion from poet to philosopher led to the unifying concept that thought and action are inextricable. It also produced a severe challenge to religion that in turn affected the other categories of life. This challenge and the restructuring of thought are the themes of the second part of Wade's study.

His method is to select the key figures in each category for elaborate treatment: Holbach and Helvétius for religion (though Voltaire and Rousseau are given much more attention); Rousseau for ethics; Diderot for esthetics; Turgot for economics; Buffon for science; and Condillac's sensationalism as the bridge to knowledge of the self. Wade brings all the major and minor philosophes into his explanation of attitudes toward thinking, feeling, and living in the context of each of the seven categories. Indeed, the strength of his examination of religion lies less in his elaborate discussion of Voltaire than in his attention to the critical influence of the clandestine treatises attacking traditional religion, by Meslier and others, on Voltaire, Rousseau, and Diderot. Steeped in Spinoza, the long-developing criticism of Christianity led to the conviction that the social and political order should not be tied to revealed religion and that a new individually or socially derived morality aimed at securing man's happiness must be developed. All the categories of life were greatly altered as a result of the onset of natural religion, but none more than politics. Taking special care to prove the seminal importance of Saint-Pierre (he was not an "amiable freak"), Wade demonstrates that the central goal of the study of politics for all the great thinkers was the creation of a secular morality.

In *Esprit Révolutionnaire* Wade tries to penetrate the "inner reality" (2: 4) of the Enlightenment, which is to say the organic unity of thinker, environment, and work using a method based on "Taine's historical criticism continued by the structure-form analysis of the 'new' critics..." (2: 3-4). He argues that there are not many disparate Voltaires, Rousseaus, and *Encyclopédies* but that the life and work of each possess organic coherence. The apparently discontinuous, diverse thoughts of Voltaire or Diderot have organic connection and build to a resolution. The spirit of what Voltaire thought, which emerged from *Candide*, was the conclusion that conscious knowing must lead to creative action; otherwise, thought is pointless. In Diderot the only difference was that the action takes place in the individual; the person thinking, rather than the society, changes. As Wade says, Voltaire carried on

combat with society in an effort to convert thought into action, whereas Diderot debated with himself. Consequently, the problem of finding the organic unity in Diderot's thoughts was Diderot's own problem. In locating the form and structure of Diderot's philosophy (as with the other figures he treats), Wade examines all the intellectual influences on him, his use of the ideas of others, notably Brucker in philosophy, his application of scientific principles to other categories of life (Wade overlooks Vartanian's study of biology and politics in Diderot), the changes that had taken place in the Enlightenment since the formative years, and the interpretations of Diderot by such scholars as Mornet, Venturi, Crocker, and Proust. He concludes that Diderot's way of knowing was encyclopedic. His thought, therefore, was ambiguous and eclectic "because life is that way" (2: 114).

The attack on religion formed the basis of the search for a new political and social order, but converting thought into reform required an adjustment between the forces of nature and the spiritual capacities of the individual, or, more concretely, between collective and individual interest. These problems were tackled by Rousseau, Morelly, Helvétius, Holbach, Raynal, Mably, and Condorcet, all of whom rate chapters in Wade's second volume. But if the spirit of the Enlightenment is the firm link between thinking and acting in the seven categories of life, then what is the relationship between the Enlightenment and the Revolution? Wade's answer flows naturally from his thesis. Out of their attack on religion, their secular morality, humanitarianism, love of liberty, and critical spirit, the philosophes spread discontent with the old order and lay the propagandistic and ideological bases for the Revolution.

Ira Wade's Enlightenment is somewhat less intellectualized than Cassirer's, less deft than Gay's, more affirmative than Crocker's, completely blind to Darnton's and Foucault's. The product of a lifetime of study and devotion, it asks us to believe that there was unity in eighteenth-century French philosophy based on the way the philosophes thought about categories of life. Yet this was an age so truculently diverse, so rampant with conflicting currents, that one wonders if the unity exists in Wade's brilliant mind rather than in the Enlightenment. The author certainly alerts readers over and over to the intellectual dichotomies that existed, often within the same figure, but each philosophe, nonetheless, ends up organically put together in ways that often seem unreal. Moreover, if we accept the proposition that the unity of the Enlightenment is based on the principle that thought is the basis of action, is it possible to contend that the philosophes wanted the same action? Wade leaves the impres-



sion that they did despite their disagreements, but he tends to slide back into the clichés about Enlightenment belief: the common quest for liberty, progress, happiness, useful knowledge, brotherhood, and reason. If these were the common goals of the philosophes, then I doubt that their philosophy tells us much about the physical or psychic life of eighteenth-century French people. I doubt that "the whole age" (1: xxiii) entrusted to the intellectual elite the task of destroying the mistaken ideas of the past and building our modern civilization. Wade has fashioned an impressive analytical method to prove his case for unity. His thorough discussion of both Enlightenment historiography and the ideas of the philosophes serves us better than the surveys with which we are bombarded. But perhaps it is less important to understand the intellectual unity of the Enlightenment, if such existed, than to comprehend people's myths, fantasies, fears, and dreams.

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CHARLES R. BAILEY. *French Secondary Education, 1763-1790: The Secularization of Ex-Jesuit Collèges*. (Transactions of the American Philosophical Society, number 68, part 6.) Philadelphia: The Society. 1978. Pp. 124. \$10.00.

Excellent work on French education in the Old Regime has been published during the past decade, culminating in the synthesis by Roger Chartier, Marie-Madeleine Compère, and Dominique Julia, *L'éducation en France du XVI<sup>e</sup> au XVIII<sup>e</sup> siècle* (1976). But there remained a need for a well-researched volume such as this on the fate of the Jesuit colleges after the Society was expelled from the more than one hundred secondary schools they had operated in France in 1762. The expulsion loosed a flood of proposals from critics of the rigid classical program in the colleges. This volume is a study of the frustration of these expectations. It does not concern itself with the drama of the uprooted professionals who had to seek employment outside of teaching; rather it focuses on the administration of thirty-eight of these schools that were under the jurisdiction of the Parlement of Paris up to the crisis of 1790.

It was not the intention of the new administrators nor of the parents of the students to make substantial changes. The traditional curriculum continued to satisfy the interested public despite occasional complaints about inadequate attention to science. Discipline was rigorous in a quasi-monastic setting; students were rarely rebellious. Theoretically all provided the same instruction, but variations in resources and the capacity to attract qualified teachers had their expected effect. The school

with the highest standards, both under the Jesuits and after their departure, was Louis-le-Grand in Paris; its prestige and wealth required special administrative arrangements and raised hopes that it might become a model college that would provide teachers for the rest—a prototype for the later *École normale supérieure*.

The transfer of the colleges did not diminish clerical control. Most of the principals and teachers were priests and the new administrators were zealous to protect the morals and faith of the students. Instead of unitary control, administration was parceled out among the parlement, the crown, municipal authorities, and boards of notables—a cumbersome system that nonetheless worked reasonably well. Since decisions on major issues followed the traditional groove, the various authorities struggled with the management of complex assets, the recruitment of competent staffs, and the choice of *boursiers*. As the Revolution approached, there is evidence of the impact of inflation and of the harsh winter of 1788-89 on the poor, but the boards remained involved with such trivia as a subsidy to a teacher in Louis-le-Grand for his volume on *The Virtuous Scholar*, which had become a best seller, and lawsuits against villages that had set up wine-presses in contravention of the schools' seigneurial rights. This evidence confirms that of the *cahiers* that the substance of education was not at stake on the eve of the Revolution. That it *was* in fact at stake was due to a few scattered suggestions that bore the seeds of a unified and secular national system. Significantly, this new structure was grafted onto the curriculum, discipline, and spirit of the secondary system of the Old Regime.

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JEAN-BAPTISTE VIALON. *La croissance agricole en France et en Bourgogne de 1850 à nos jours*. (Dissertations in European Economic History.) New York: Arno Press. 1977. Pp. 278. \$34.00.

Using all the tools of the cliometrician short of the computer itself, Jean-Baptiste Viallon has probed nearly every feature of French agricultural growth since 1850 that will yield to quantitative analysis. His range of issues is so staggering, in fact, that the narrative becomes rather dense and suffers from a rigid chronological presentation of evidence on one topic after another. For economic historians and specialists of rural history in particular, however, Viallon's findings will be of some interest. He has meticulously calculated over the entire 120-year span composite and separate indexes of production for fifteen basic agricultural commodities. He ana-

lyzes for the same period the factors of production (land, labor, capital, technology) and the role of market outlets. In so doing he seeks to show that agricultural growth was hindered primarily by forces external to farming, specifically by sluggish industrialization, slow urbanization, and a weak national market. These problems, rather than a "backward" agrarian base, became the critical economic constraints in the late nineteenth century, Viallon suggests, because by then only a dynamic industrial economy could have stimulated further farm production, built new channels of national and foreign trade, pulled excess labor from the land, and provided the farmer with both capital and incentive to adopt new techniques.

To test and refine his thesis, Viallon systematically compares evidence for the region of Burgundy with that for France. The regional persistence of the traditional grains and vine culture, and abortive spurts of growth in the 1850s and in 1892–1912, suggest that in Burgundy, as in France, real agricultural development required a healthier national economy than France possessed. Yet permanent stagnation is far from what Viallon finds. Rather, from the 1880s onward for a combination of reasons, "agricultural development was subordinated to industrial development" (p. 214). Not until the period 1955–70, marked by strong industrial expansion and the rise of the Common Market, was the French agricultural revolution finally achieved.

Viallon's evidence is always suggestive; on many issues it is compelling. He provides new perspectives on much of the recent work on French economic growth. And, while he acknowledges a debt to such authorities as Lévy-Leboyer, Toutain, and Marczewski, his view that sluggish industrialization retarded agriculture will challenge theirs in several respects. His book also provides valuable observations on such topics as the persistence of the family farm, the role of viticulture, the *crise agraire*, the Méline tariff, and the impact of government economic policy. Yet his major point will appear to many readers to minimize the structural problems within the agricultural sector. This issue is compounded because Viallon passes over much evidence on the peasant labor force in his principal source, the massive *Statistiques agricoles*. He does not discuss land tenures or the impact of rents on profits or evidence on yields. Moreover, showing that the industrial sector failed to exert sufficient demand to stimulate agriculture does not prove that, if industrialization had been more rapid, agriculture would have developed rapidly too (as he implies for the 1880s–90s): other factors, including structural limitations within agriculture, could easily have intervened farther up the line of growth. Finally, had Viallon selected a region in the center or southwest, rather than Burgundy, his results might have indicated that agri-

cultural retardation was more profound than he appears willing to admit.

These comments do not diminish what is by any standard a thorough, fresh, and suggestive analysis that will on a number of issues refine the questions and terms of discussion for all who will follow. The 270-page statistical appendix alone makes this an indispensable reference book. And its appearance in this country is a hopeful sign of easier access to the best specialized French research. Originally a *thèse de troisième cycle* presented in 1976 at the University of Dijon, which has long been an important center for agrarian history, this book is a photo-offset reproduction of the original typescript published in the "Dissertations in European Economic History" series.

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LESLIE DERFLER. *Alexandre Millerand: The Socialist Years*. (Issues in Contemporary Politics. Historical and Theoretical Perspectives, number 4.) Paris: Mouton. 1977. Pp. x, 326.

Alexandre Millerand has always enjoyed a fame or notoriety stretching beyond the confines of French and European history that probably exceeds his actual political importance. As the first socialist to enter government, he became a symbol of the reformist socialism that has virtually dominated West European politics in the twentieth century. Together with the self-proclaimed revisionist Eduard Bernstein, Millerand came to personify the reformist betrayal of Marxist revolution. Yet, perhaps because of his symbolic importance, Millerand has never found a serious biographer to explain the circumstances and meaning of his reformist socialism. Leslie Derfler has filled the gap with a lucid and well-written biography of Millerand in his socialist years.

Derfler dispels the myth of socialist betrayal by showing that it was French socialism rather than Millerand that changed political direction at the turn of the century. When Millerand entered the Waldeck-Rousseau government in 1899, he merely attempted to put into practice the reformist program that he shared with the other founding fathers of French socialism—Jules Guesde, Jean Jaurès, and Edouard Vaillant. While Millerand held steadfast to this program, they felt compelled by his action—and by the adverse effect it created in the trade unions—to return to the more directly revolutionary approach they had abandoned in the parliamentary 1890s.

The author draws a convincing portrait of a socialist who was above all a lawyer-politician and

member of the governing elite that included his mentor Georges Clemenceau, his patron René Waldeck-Rousseau, and fellow reformist Aristide Briand. With his square mind and body, Millerand was the embodiment of the pragmatic and opportunist politicians who ran the Third Republic. External events rather than internal processes molded his socialism. In contrast to the passionate Jaurès, his approach to socialism was cold and juristic. He began his socialist years as an advocate for striking workers and ended them as a protégé of the corporate lawyer Waldeck-Rousseau and attorney for the religious congregations. He was an incorrigible independent who never hesitated to break with his closest political allies—be they Radical, Socialist, or Opportunist—when circumstances warranted.

By entering the government along with General Gallifet, the hated “butcher” of the Commune, Millerand did not betray French socialism. His act of republican defense during the Dreyfus Affair was initially approved by party leaders. As the only socialist in a conservative cabinet, his accomplishments were considerable. The legislation that he sponsored for compulsory labor arbitration, retirement benefits, and the limitation on working hours was drawn from the same socialists who now opposed it. He gave a fillip to the trade unions that spurned his help. No wonder that Jaurès continued to praise his record even after he left the socialist group. He was at the very least a pioneer of the welfare state.

As illuminating as it is, Derfler’s biography suffers from some of the limitations of the genre. The background material on socialism and the labor movement is mostly derivative and dated. No reference is made to recent work in social history by Michelle Perrot, Jacques Julliard, Joan Scott, Bernard Moss, and others. When Derfler does utilize primary research on French socialism, he places undue reliance on the judgments of police spies, who invariably viewed ideological quarrels in terms of personal rivalries and jealousies. More seriously, though all the elements for it are present, Derfler does not attempt an overall analysis of reformist socialism in relation to other socialist currents. Such an analysis would reveal that many currents, including the Marxist, suffered from the same weaknesses as Millerand’s—an excessive individualism, dissociation from the trade unions, and disjunction between collectivist theory and reformist practice.

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BENJAMIN F. MARTIN. *Count Albert de Mun: Paladin of the Third Republic*. Chapel Hill: University of North Carolina Press. 1978. Pp. xix, 367. \$20.00.

Perhaps the chief usefulness of writing the history of the French right between 1870 and 1914 is to confirm humanity’s infinite capacity for enduring the demonstrable futility of its own efforts. Certainly Albert de Mun, a principal leader (though in France of course not the only leader) of the right throughout this period, emerges from Benjamin F. Martin’s biography as a generally appealing figure, and one rather simply explained in psychological terms. Born of an old chivalric family, he was a devout and Jesuit-bred cavalryman who dedicated his life to noble causes: social reconciliation and the reinvigoration of France’s religious and patriotic faith.

Yet de Mun’s career was a succession of galling disappointments. His social Catholic movement soon dwindled into insignificance because socialists mistrusted Catholics and most Catholics were not socialists. He would have liked to see a king like Henry V restored but soon concluded that it was impossible and that by the last decades of the nineteenth century the best the Church could hope for was accommodation with more conservative Republicans. Yet, though he was often pilloried by regenerate royalists for this inclination to compromise, he himself took a leading role in the right’s repeated disastrous efforts to escape its dilemma by force. A regional leader of Boulangism, de Mun also turned away from the *Ralliement* to become an anti-Dreyfusard and denouncer of the “Jewish syndicate,” though the defeat of Boulangism and the vindication of Dreyfus hastened the consolidation of the anticlerical Republic. Only in supporting Poincaré’s efforts to strengthen French preparedness before 1914 can de Mun’s politics finally be classified, with mournful irony, as successful. For his kind of Frenchman vanished forever with the young white-gloved lieutenants from Saint Cyr bravely advancing upon the German guns of August. Appropriately, de Mun himself died in 1914 in the midst of efforts to provide the secularized French army with battlefield chaplains.

Martin’s reconstruction of this life of exemplary failure is well organized, based on a thorough command of the relevant accessible sources, and lucidly written. It might have been considerably shorter if the author had not felt it necessary to explain in detail each of the successive crises of the Republic with which de Mun had to deal. One may wonder if the author’s essential characterization of de Mun as a literally quixotic figure always serves his interpretive needs. In cooperating with men like Méline and Poincaré, despite the fulminations of Catholic intransigents, de Mun sometimes showed supremely pragmatic skills, as Martin himself sometimes seems to acknowledge. And was it so surprising that de Mun allowed himself occasionally to be tempted away from this soul-wearying, patient realism by

hopes that a *deus ex machina* like Boulanger or the Dreyfus affair would end his basic dilemma that in a democratic France fervent political Catholics were a dwindling minority of the electorate? What other hope had he?

Such minor criticisms apart, Martin's is an excellent example of the kind of biographical monograph that Frenchmen and Americans have been writing to explain the Third Republic's politics since before the regime ended in 1940. The principal characters of the drama are Parisian politicians, and their dialogue has been meticulously reconstructed from their letters and editorials. Perhaps for his next project Martin could be tempted away from these familiar salons to the farms and workshops where, despite the recent stimulus of Eugen Weber's *Peasants into Frenchmen*, the Third Republic's real *terra incognita* remains largely unexplored. Now that we know as much as we shall ever need to about men like de Mun, it is surely time to find out, for example, who belonged, where, and why, to organizations like his *cercles* of workers, and who voted, where, and why, for his *Action Libérale Populaire*. Biographies of the Third Republic's politicians are by now perhaps too abundant; biographies of their electorate are still much too rare.

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MARJORIE GRICE-HUTCHINSON. *Early Economic Thought in Spain, 1177-1740*. Boston: George Allen and Unwin. 1978. Pp. 189. \$24.75.

This handsomely printed, well-written study is a welcome addition to the history of economic thought. In 1952 Marjorie Grice-Hutchinson introduced the extraordinary group of late scholastic monetary theorists who flourished at the University of Salamanca in the sixteenth century. In the first part of this book, she traces the long roots of their work into the Spanish past, a past that included Jewish, Muslim, and Christian works on money and commerce; Roman, canon, civil, and early Spanish law; and the works of ancient Greek philosophers preserved in Arabic commentaries.

Common to all of these sources was a search for just definitions of money and value. Utility, as perceived by an item's potential buyers, was widely agreed to define market value. Money served only to facilitate exchanges by indicating or storing that value and was not a legitimate source of profit, except under special conditions, in any of the three religious doctrines. Gradually, the needs of commerce bred tolerance for tactics to avoid the letter of usury prohibitions, first among Jewish theorists, then among Muslims, then among Christians. The

School of Salamanca inherited not only this complex Spanish legacy but also the works of other Europeans.

In the sixteenth century the vast influx of bullion from the New World, inflation, and the booming European economy forced Spanish scholars to revise and refine medieval theories about the economy: the utility theory of value; the relation between money and prices; and banking and foreign exchange. From these refinements came the first clear statement of the quantity theory of money in relation to prices, by Martín de Azpilcueta in 1556. Another group of writers dealt more with reality than with theory, clearly analyzing the Spanish economy from 1500 to 1740 and focusing on the pernicious effects of New World riches, on government deficit spending, currency manipulation, and high taxes, and on the incursions of foreigners into the economy. Grice-Hutchinson summarizes the work of dozens of these political economists in her final chapter.

The brief sections that deal with economic history rather than with economic thought are unsatisfying, since the author often relies on general works that lack a sophisticated view of how population, agriculture, industry, and trade fit together over time. But that does not detract from the book's considerable merits. It should be widely read, not only by those interested in Spanish history but also by those concerned with the evolution of economic thought during Europe's transition to a capitalist economy.

CARLA RAHN PHILLIPS  
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CARLA RAHN PHILLIPS. *Ciudad Real, 1500-1750: Growth, Crisis, and Readjustment in the Spanish Economy*. Cambridge: Harvard University Press, 1979. Pp. viii, 190. \$16.50.

"In La Mancha, the wine is good and the girls are bad," wrote a French traveler in 1672, nostalgically recalling the red Valdepeñas grapes and the nubile Manchegan whores. Both were at their best in the provincial capital, Ciudad Real. Carla Rahn Phillips's slim volume (only 116 pages of text; the rest contains appendixes, notes, and index) describes some of the changes and continuities of life in that medium-sized city of New Castile during the early modern period. There is a survey of the urban population and of its economy, an analysis of landholding patterns and political elites, and a study of royal taxation in the city. This last section (pp. 76-94) is perhaps the best, showing at a local level the serious difficulties facing the royal treasury as it attempted to finance Habsburg imperialism. As taxes



increased, arrears of unpaid assessments mounted—payment of taxes due in the 1640s was delayed by an average of fifteen years—and to fill the resulting deficit the government was forced either to adopt tougher methods of collection or to impose yet more taxes. Corruption also played its part in creating a fiscal crisis: one tax collector in 1621 even deducted from official funds the mourning suit he bought for King Philip III's funeral.

Alas, the significance of data like these is seldom considered. Phillips never explains just how the taxes of Ciudad Real were collected. We are informed, tantalizingly, that after 1640 new levies were collected by "stringent and inequitable means"—an important change that might help to explain the "Green Banner" revolts of 1647–52—but the new methods are not specified. Likewise, there are no global figures on how much individual taxpayers had to provide for the fisc; nor is it explained how, precisely, overtaxation led to the decline of the Habsburg empire—we are just told that it did (p. 93). The same lack of a wider perspective mars the important demographic chapter. The parish registers show a catastrophic fall in the city's baptisms and marriages in the decade after 1604 to less than half their former level; recovery came only after 1640. Yet this crucial development, so significant for an understanding of the "decline of Spain," is never properly explained. Admittedly, the expulsion of about two thousand five hundred *moriscos* after 1609 removed perhaps one-quarter of the city's population; but the decline predated the expulsion, and it far exceeded 25 percent. Perhaps the plague or some other epidemic was to blame? But Phillips offers no evidence, for, "in the absence of mortality figures for either parish in this period, there is no way to know if the plague of 1596–1602 affected Ciudad Real" (p. 28). No way? Does not a single chronicle, diary, or letter from Ciudad Real survive for these years? And, even if not, the regular reports sent by the city authorities to the various royal councils (especially the councils of war and finance) are available in bulk, as are the deliberations of the city council. Surely these sources contain information on changing local conditions—on the advent of plague at least? It is, however, impossible for the reader to discover from this book whether such archival material has been consulted, for, although the eleven-page bibliography ranges from *Don Quixote* (early modern La Mancha's most famous citizen) to articles on Hungarian agriculture, there is unfortunately no discussion of the primary sources consulted.

Small is indeed beautiful, even where scholarly monographs are concerned; but one can flee too far from the massively thorough studies of French urban historians such as Bartolomé Bennassar or

Jean-Pierre Le Flem. A mere 116 pages is not enough to deal adequately with the history of a town of ten thousand people over two and a half centuries. It may be true, as the jacket of this book proclaims, that "Through local studies . . . we are gradually obtaining a much clearer picture than we hitherto have had of economic and social change during Spain's golden age." But not much of the picture can emerge through a study as summary as this.

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IRIS M. ZAVALA. *Clandestinidad y libertinaje erudito en los albores del siglo XVIII*. (Letras e ideas, number 12.) Barcelona: Editorial Ariel. 1978. Pp. 459.

Iris M. Zavala has ransacked Spanish libraries and archives for unknown publications and clandestine manuscripts dating from about 1680 to 1760 and has mined the secondary literature on the period. Based on this vast effort, her book gives a fresh vision of the Spanish mind during the recovery from the seventeenth-century nadir. Her picture looks something like this. A new spirit of innovation or "libertinaje erudito" arose first out of an interest in scientific and medical advances at the end of the seventeenth century (as in France, the great comet of 1680 gave grist to the skeptics of astrology), and the advent of the Bourbons encouraged those who looked to foreign inspiration. After 1700, clandestine poetry and tracts attacked the royal ministers and even the kings themselves. Much of this was traditional in tone, defending the Habsburg pretender and an old order, but Zavala emphasizes that some social criticism shows that the spirit of the *arbitristas* lived on under cover to inspire the Spanish Enlightenment. Meanwhile a legitimate press spread its products to an ever broader public made up largely of petty bourgeois who lacked an intellectual or university background. Almanacs were their favorites, replete with tales and prognostications. The most popular of these was the *Piscador de Salamanca* (1719–67), issued by Diego Torres Villarroel, the most prominent figure in the book. Thus, this "threshold of the Enlightenment" prepared a new critical secular spirit and a new public for the better-known flowering under Charles III.

Zavala's new information definitely calls for a re-evaluation of the period, more active in letters than we had realized, but I doubt that the accepted picture of the Spanish Enlightenment is modified as much as Zavala's wording often implies. Most of the critical literature, clandestine and other, appears conservative or reactionary, while the writings



that she calls *arbitrista* in spirit usually go little beyond commonplace strictures on wealth, idle nobles, and injustice, too vague for evidence of a clandestine tradition of analytic criticism. Hoyo Sotomayor, a remarkable nobleman whom Zavala has uncovered, does cite some *arbitristas* by name; for others the inspiration was often Quevedo's stoic satire or ideas from abroad. Most of the works cited are later than 1725 and those of social criticism after 1735, well after Feijóo had begun to stir Spanish minds, when one would expect to find new ideas voiced. Before this time the current of progressive thought still looks pretty meager, although it is now clear that Feijóo did not begin it.

A separate question is Zavala's insistence that this critical literature represents the growth of a "bourgeois mentality" in a new "consumer society." She seems inspired by an outdated view of the Enlightenment. No doubt the reading public was growing. To judge from Torres Villarroel, however, held up as an example of a bourgeois entrepreneur appealing to a bourgeois public, the bourgeoisie was not the patron of progressive doctrines, for he was basically reactionary, an enemy of Feijóo, and a defender of privileged estates. The social origins of the writers were about what one would expect a century earlier, bureaucrats, professionals, clergymen, and some nobles. The threshold of enlightenment does not seem to be the product of social change, and the Spanish Enlightenment still appears to have depended heavily on foreign inspiration and to have become a broad movement only after the government of Charles III demonstrated active support. Perhaps Zavala does not really intend to propose the contrary but to modify our conception of the earlier period and to resurrect its writers from oblivion. Although some of her themes may be questioned, in this undertaking she has succeeded.

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MIGUEL ARTOLA. *Antiguo Régimen y revolución liberal*. (Ariel-Historia.) Barcelona: Editorial Ariel. 1978. Pp. 318.

The publication in 1962 of Pierre Vilar's economic and social analysis of eighteenth-century Catalonia began a revolution in the writing of Spanish history that has developed still more vigorously in the freer intellectual atmosphere of the 1970s. The works of Gonzalo Anes, Josep Fontana, Pablo Fernández Albaladejo, and others testify to the displacement of conventional political history in favor of the search for the economic roots of Spain's nineteenth- and twentieth-century difficulties.

Miguel Artola, highly regarded for his earlier

work on the Napoleonic and early liberal periods, has written a Marxist interpretation of the relationship between the economic tensions of eighteenth-century society and the emergence of bourgeois liberalism that first took institutional form in the Cortes of Cádiz (1810). Artola argues that in spite of the preindustrial character of the old regime economy and the extensive landholdings of the church and nobility, control of the means of production and market forces took a distinctly capitalist form that generated class conflict between an incipient agrarian bourgeoisie and traditional noble and ecclesiastical property interests. He traces the struggle over agricultural leases, rents, seigniorial rights, the tithe, and freedom of movement in the marketing of agricultural products during the second half of the eighteenth century. He views this conflict as a necessary prelude to the dismantling of the old regime economic structure begun by the Cortes of Cádiz, continued by the liberal revolution of 1820–23, and concluded after 1834 by triumphant liberalism.

Artola argues his case with methodological sophistication and impressive documentary sources. Yet the relative neglect of social, cultural, and political aspects in favor of an economic interpretation of the destruction of old regime society leaves this study less comprehensive than it might have been. A more detailed description of the composition, economic resources, and occupations of the preindustrial bourgeoisie of the eighteenth and early nineteenth centuries would have been helpful, although the still lamentable lack of regional and local studies may account for its absence. But this is a valuable work of interpretation that should be read by all students of the period.

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IGNACIO OLABARRI GORTAZAR. *Relaciones laborales en Vizcaya (1890–1936)*. Durango, Spain: Leopoldo Zugaza. 1978. Pp. xxvii, 532. 1,600 ptas.

According to the author, this study is a pioneer effort to analyze the system of industrial relations in the Basque province of Vizcaya, a region whose economy and labor force were transformed by the rapid growth of the iron and steel industry after 1890. Using archival sources (including those of the leading steel producer, *Altos Hornos de Vizcaya*, as well as those of a wide variety of employers' and workers' associations), together with official publications and the press, Ignacio Olabarri describes the organization, policies, and activities of the principal participants in the industrial system, the various modes of interaction between them—strikes, negotiation, and government arbitration—and the "work rules" that emerged from such interaction. In

a final section, the author traces the evolution of the system of industrial relations between 1890 and 1936 and concludes that the breakdown of the system was not the result of an inadequate institutional framework but rather the product of political, cultural, and economic factors external to the system.

Some of this material duplicates the work of Juan Pablo Fusi, whose *Política obrera en el País Vasco, 1880-1923* appeared in 1975. What is new is Olabarri's effort to place the labor movement within a broader framework that also includes employers and the state. Although the book is overly long and occasionally repetitive, the text, and especially the extensive footnotes, contain a great deal of new material on the internal governance and changing strategies of both employers' associations and labor unions in the region. Also new are the extended sections on the Catholic and Basque nationalist workers' syndicates, which, along with the anarchosindicalist unions, became successful rivals of the socialist *Unión General de Trabajadores* after 1930. Olabarri argues that this institutional diversity grew out of the heterogeneous needs and outlooks of the Vizcayan working classes, not all of whom responded positively to the combination of revolutionary rhetoric and moderate tactics offered by the UGT.

The author's insistence on the pluralism of the Spanish labor movement represents a revisionist viewpoint in the field of Spanish labor history, which has been dominated by a Marxian model that presupposes the linear development of class consciousness and the reality of class conflict. The underlying assumption in Olabarri's model of industrial relations is that conflict arises from the malfunctioning or improper institutionalization of a system designed to prevent or resolve it; when conflict continues to intrude in a properly functioning system, it is for political reasons that have little to do with the elaboration of the work rules that regulate the interaction of labor and capital.

This kind of model has its own shortcomings. Even though Olabarri has provided a welcome corrective to an oversimplified view of the labor movement, he begs the question by dismissing as irrelevant to his analysis those Spanish workers whose affinity for political or revolutionary strikes kept them outside the system. Olabarri's systematic global analysis needs to be complemented by and related to an analysis of the attitudes and experiences of the workers whom the system was supposed to serve. Such a synthesis would lie at the intersection of social history, political history, and the institutional history we have here.

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A. R. DISNEY. *Twilight of the Pepper Empire: Portuguese Trade in Southwest India in the Early Seventeenth Century*. (Harvard Historical Studies, number 95.) Cambridge: Harvard University Press. 1978. Pp. ix, 220. \$18.50.

In the aftermath of World War II and, more notably, in the last two decades, Australian scholars have shown a growing interest in Asian studies. The present work dealing with a crucial but declining phase of Portuguese trade with Southwest India, however, focuses more on the Portuguese than the Indian dimension, reminding one of the Dutch scholar J. C. van Leur's admonition that Asian history has too long been observed "from the deck of the ship, the ramparts of the fortress, the high gallery of the trading house." A. R. Disney's study ranks with the very best scholarship on Portuguese activity in Asia, in the same category as the excellent research of M. A. P. Meilink-Roelfsz, in carefully outlining and analyzing the trading patterns of the time. In doing so, he has, like Meilink-Roelfsz, exploded the myth that the Portuguese were an impractical people striving for the greater glory of their monarch and proselytization at the expense of trade and profit. It is true that the Portuguese could not adequately meet the challenge posed by the English and the Dutch in the seventeenth century, but this was not totally for want of trying. In the late 1620s, the Portuguese attempted to combat the crisis by launching their own India Company. Disney elaborates, for the first time, the details of the enterprise, how it foundered on the rock of rampant corruption, and the general incapacity to modernize commercial operations.

Although the Portuguese traded in a wide variety of Asian products—rice, saltpeeter, timber, and ivory, to mention a few—the pride of place went to pepper, constituting 80 percent of the total trade. Pepper was mostly grown in Malabar and Kanara and bought by the Portuguese from Indian middlemen in Southwest Indian ports. Disney has well outlined the procedures from the point of purchase, warehousing, and loading to their passage to Portugal. Very little, though, has been said of the agents through whom the purchases were made, even though the most significant primary source on the subject, Francisco da Costa's records, gives relevant details. On the other hand, unlike da Costa, Disney has supplied details of the cultivation practices and land tenure, for the most part guessing backward on the basis of the early nineteenth-century multivolume account of Francis Buchanan's travels in Mysore, Malabar, and Kanara. For some unknown reason, though, Disney has limited his description to Malabar.

No great piece of research is devoid of minor flaws. Ramaqueny (pp. 34, 110) should be Rama

Queny; he was not a Banian but, like most of the traders in Goa then as now, was a Saraswat Brahman. Goa has been spelled Gowa (p. 27), and the Goan community described as "canarins," an antiquarian term that may be used only for describing people of Kanara, south of Goa. Disney should have perhaps told us something about the relationship of Indian pepper trade with that of Southeast Asia and how prices were fixed in the two areas. Some comparison of trading practices of Portuguese in West India and the Dutch in Java and Ambon would have contributed further to the better understanding of the decline and demise of the Portuguese pepper empire.

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L. DE JONG. *Het Koninkrijk der Nederlanden in de tweede wereldoorlog*. Volume 7, *Mei '43-juni '44*; volume 8, *Gevangenen en gedeporteerden* [The Kingdom of the Netherlands in the Second World War. Volume 7, May '43-June '44; volume 8, Prisoners and Deportees]. Each in two parts. (Rijksinstituut voor Oorlogsdocumentatie.) 's-Gravenhage, The Netherlands: Martinus Nijhoff. 1978. Pp. vii, 688; vii, 689-1442; vii, 578; vi, 579-1018.

The seventh volume of L. de Jong's history of the occupation covers the period from the end of the 1943 April/May strikes, the major turning point in the history of the occupation, to June 1944, the month of the Allied landings in Normandy. In this volume he describes "another Holland" (p. 683), a country in which the spirit of resistance made itself felt among wide circles of the population who up to this point had tended to go along with the German occupation regime. De Jong attributes this greater militancy primarily to the 1943 strikes, but, out of a comparative perspective, I would tend to put somewhat greater weight on the change in the fortunes of war after El Alamein and Stalingrad, since similar shifts can be identified in other occupied countries of Western Europe.

In the seventh volume de Jong deals with a variety of topics. He first discusses the deteriorating economic situation, pointing out that food and other commodities became increasingly scarce but that living conditions for the majority of the people remained tolerable until September 1944. He then proceeds to describe the closing phases of the deportation of the Jews when the Germans tried to remove the remainder of the Jewish population to the East (almost one half of those registered as Jews had already been deported). Again, as in volume six, he stresses the cooperation of Dutch government offi-

cials and of the Dutch police in the deportations and the relative silence of the underground press. He documents more concretely than any previous literature the unwillingness of almost all concerned to believe the often quite specific and graphic stories of the mass shootings and gassings brought back to the Netherlands from Eastern Europe by a variety of individuals. He reminds us how unbelievable such stories were to those in the pre-1945 world. And he ascribes a switch in the attitude of Seyss-Inquart toward the deportations after September 1943 to the desire of the *Reichskommissar* to establish an alibi that would serve in future negotiations with the West.

Most of the remainder of the volume is devoted to the underground movement and to the measures the Germans took to combat the Resistance. A special chapter describes the aid to people in hiding, the greatest accomplishment of the Dutch Resistance. The volume ends with a portrayal of the gradual demoralization and radicalization of the Dutch Nazi movement and with the German preparations for the expected invasion.

This installment of the "History" offers a somewhat unusual contrast in that its first part deals with the distress and suffering of the Dutch people as economic conditions deteriorated and with the destruction of tens of thousands of Jewish Netherlands, whereas the second part describes how growing segments of the Dutch people found a sense of purpose and hope by embarking on the path of resistance. It also shows clearly how, together with the changes in the war situation, the radicalization of German policies, especially the forced labor draft, contributed to this rise in Dutch resistance.

The eighth volume of de Jong's work deals with the fate of prisoners and deportees from 1940 to 1945. It is unique among the volumes published so far in its topical approach, which cuts across the entire occupation period. That concentration and the nature of the topic make the eighth volume easily the most moving and "upsetting" part of the story de Jong has told so far, even though the professional historian may not find many fundamentally new conceptions or insights that add to the existing literature.

De Jong begins with a description of the SS empire as it functioned all over occupied Europe. Since this is not an area of original research for him, he bases his story on well-known, largely European sources. He then moves on to deal in separate chapters with prisoners of war, hostages, political prisoners, such as members of the Resistance, and with a description of the concentration camp experience as such, using generally familiar as well as specifically Dutch sources. In the second part of the eighth vol-

ume he describes the concentration camps located in the Netherlands and devotes a special chapter to the fate of Jewish deportees. He concludes with a review of assistance rendered from the outside to these various categories of prisoners.

In this volume above all others, de Jong's original journalistic background and his superb mastery of the language provide one of the most harrowing descriptions of the SS terror and its victims that I have read so far. One quality of de Jong's writing that makes this such a powerful volume is his frequent and highly effective employment of eye-witness accounts, which he uses to maximize the impact of his story, without losing his professional touch or objectivity, or without ever becoming maudlin or sentimental. His essentially narrative rather than analytical approach and his unflinching attention to detail reminded me of nineteenth-century historical writing in the German and English traditions.

Insights new to this reviewer included the extent to which even a highly privileged group such as the "Indian hostages" who were arrested in 1940 and housed in relatively comfortable circumstances experienced great pressures and fears and, on the other hand, the manner in which these men (and to a lesser extent the interned commissioned officers) put their time to good use. Also striking is de Jong's observation that in one sense the Germans helped to create a basis for postwar reconstruction by interning so many members of the Dutch elite. The years spent together in the camps made possible extended discussions about postwar problems which led to the formulation of reconstruction programs that were implemented after liberation. In this context de Jong suggests that the qualities Schermerhorn displayed as *Vertrauensmann* (spokesman) of the Indian hostage group may have contributed to his selection as the first postwar prime minister of the Netherlands. This example gives us an intriguing illustration of the intimate connection between life under the occupation and that in the immediate postwar period.

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ØYVIND ØSTERUD. *Agrarian Structure and Peasant Politics in Scandinavia: A Comparative Study of Rural Response to Economic Change*. Oslo: Universitetsforlaget; distributed by Columbia University Press, New York. 1978. Pp. 278. \$15.00.

This exhaustively researched study is concerned with "the general contrast between the peasant and the non-peasant world," and its purpose is "to relate the economic specificity of peasantry to peasantry as a historical phenomenon and to the political soci-

ology of peasant behaviour" (p. 19). Its scope is strictly limited to Denmark, Norway, and Sweden and to the subject matter indicated in the title. Therefore one can expect nothing on home life or crops and cultivation. With care Øyvind Østerud defines his terms and explains concepts, methods, and the variations among the three countries by period and country. Comparisons with Continental conditions are not attempted and outside influences on matters such as the enclosure movement are referred to only in passing. Scholarly caution produces a sometimes ponderous style, but the work is clear and readable. Within its tight limits it is a highly knowledgeable treatment of the subject.

Ancient similarities among the Nordic countries had yielded by the eighteenth century to startling differences, and the main thrust of the volume is to explain these developing differences. Agrarian politics and national legislation emerge as primary causes. Østerud emphasizes one common element for all Scandinavia: the developing market economy of the nineteenth century and the replacement by monetary criteria of political status based on landholding. Geographers will surely object that factors like climate, soil, and distance from markets are given too little attention. Lengthy consideration is given to patterns of land tenure. Within each country ownership structure was "highly conditioned by other trades—like forestry, mining and fishing—and the Norwegian crofter system expanded to supply the labour force of the new peasant proprietors" (p. 137). In Norway ownership was often based on rental value, and distinctions between tenants and freeholders were vague. On the same land one person might possess rights in fishing and hunting, another grazing rights, a third rights to certain types of wood, and a fourth rights to other types of wood. One section is summed up as follows: "In Norway, the peasant society as a whole was faced with a dominant world of public officials and commercial bourgeois. The Danish scene was still controlled by the landed interests of big estate owners, while the privileged nobility in Sweden remained a provocation to all non-noble interests" (p. 137).

Norway receives a more nuanced treatment and about twice the space devoted to either Denmark or Sweden—Norway represented greater complexities. As for Sweden Østerud attempts to minimize the effects on the nobility of the seventeenth-century "reduction." He neglects two relevant aspects of this antinoble and propeasant action: as Eli Heckscher has said, it saved the peasants "from the danger of losing both social and political independence, and the basis was laid for their following rise and ultimate position as ruling class." Furthermore, the nobles' loss of landowning power made them depen-



dent for prestige and livelihood on administrative positions in government, with far-reaching effects on politics and society.

Much more is here—on peasant antitax outbreaks, the Norwegian *odelsrett*, savings banks and granary funds, the common interests of peasants and kings, the processes of change. It is a rich mine of dependable information and insights.

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EDWIN TORKELSEN. *Christianiae dom-kirkes bog, 1632-1677* [Christiania Cathedral-Church Registers, 1632-77]. (Norsk Historisk Kjeldekrift-Institutt.) Oslo: Forlagsentralen, for Kjeldekriftfondet. 1978. Pp. 179.

This account book from the Church of the Holy Trinity—the cathedral church of Christiania (now Oslo)—contains a list of building materials used in the construction of the church; the names of the original pew holders, the sites of the pews, and the amounts paid for them; and a list of burial fees and the church wardens' accounts. These documents account for about a third of the text, the rest being given over to a lengthy introduction and detailed notes.

The editor, Edwin Torkelsen, has taken upon himself three tasks. First, briefly, he describes the documents; second, he tries to identify the different hands that produced them (an excellent piece of source criticism this, a really fine piece of detective work); and, third, he analyzes the administration of the church, focusing on the three principal sets of actors—the church wardens, the church inspectors, and the chapter.

The church wardens appear to have been responsible for the day to day receipts and disbursements of the church. The principal income was from burials, and, at one time, if a burial was not paid for, the church warden and also his heirs had to make good the church's loss. By the end of the period this was, however, no longer the case. The church inspectors were initially responsible for supervising the building of the church. Once this was done they seemed to have had little to do and, indeed, for a number of years were either under strength (their complement was supposed to be four) or nonexistent. Later, their main role seems to have been that of allocating pews. A somewhat comic wrangle over an attempt to squeeze eight women into a seven-woman pew is used to good effect by the editor to show the balance of power and the rivalries within the church administration. Finally, the chapter had the task of making church appoint-

ments, authorizing expenditures, and auditing accounts. Torkelsen shows how the chapter accommodated itself to the post-Reformation situation by a process of "adjustment and cooperation." Indeed, the main thrust of his introduction is to expose the continuities and the changes in church-state relations at the local level, one hundred years after the Reformation.

The period covered by the accounts included the plague year of 1654 when one thousand of Christiania's three thousand inhabitants were said to have died. Whether true or not, there is little doubt that the plague was good for church business, with a sharp rise in income not only from burials but also from the charges levied on people inheriting pews.

The editor and publishers of this book can be deservedly proud of their achievement. It is a pity, however, that it lacks an English summary.

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HANNE ERIKSEN. *Partiet de Uafhængige, 1953-1960* [The Independent Party, 1953-1960]. (Odense University Studies in History and Social Sciences, number 51.) Odense: Odense Universitetsforlag. 1978. Pp. 194. 40 KR.

Hanne Eriksen's essay, *Partiet de Uafhængige, 1953-1960*, describes the activities of a few individuals who sought to develop an unwavering parliamentary opposition to socialist legislation in post-World War II Denmark. Initial financial and ideological stimuli for this obstructionist campaign came from *Højres Fond* (Foundation of the Right), a small interest group with reactionary tendencies. The wealthy landowners who dominated this group had become disillusioned with existing leadership in the bourgeois parties, *Venstre* (the Left) and *Det konservative Folkeparti* (the Conservative People's Party), after several welfare-state proposals were enacted and the upper house of parliament was abolished with bourgeois support. Preferring to operate clandestinely, *Højres Fond* established "committees of concerned citizens" to lure the existing bourgeois parties away from compromise politics and back to "true conservative" principles. In the spring of 1953, these select committees found an able and popular ally in Knud Kristensen, a former prime minister who had bolted *Venstre* when the party repudiated its previous insistence on the annexation of southern Schleswig.

When indirect pressures failed to alter the stance of the established bourgeois parties, Kristensen and his new colleagues formed their own bourgeois party, *De Uafhængige*. But two disruptive problems



emerged after this splinter party failed to garner enough votes for inclusion in the 1953 *Folketing*. First, differences between Kristensen's conservatism and reactionary interests in Højres Fond became so divisive that the party waged an ineffective campaign during the 1957 elections. But Kristensen's hand-picked successors eventually gained undisputed dominance over Højres Fond and sufficiently reorganized the party at the local level to win six seats in 1960. The second problem, however, proved more intractable: should De Uafhængige work with the other bourgeois parties for the actualization of shared principles or pursue an independent course whenever possible? Eriksen's thesis is that failure to establish itself as a viable alternative to the major bourgeois parties led to De Uafhængige's electoral demise after 1966.

Eriksen's material on the formative stages of this splinter party is original and provocative. But in her preoccupation with the minutiae of party intrigue at the leadership level, the author slights an essential subject—the anxieties experienced by the larger bourgeois electorate in a society undergoing socialization, both before and after 1960. As a consequence, Eriksen does not substantiate her thesis. The explanation appears to be inexperience compounded by hasty editing. Although her archival research on the party's leadership is thorough, the interviews she conducted yielded insufficient and superficial results. In addition, she makes troublesome mid-course corrections in her thesis to accommodate her focus on elites instead of revising the entire manuscript. The writing is choppy and uninspired; the organization is plagued by repetitiveness and an overabundance of tangential remarks. And the footnotes are so profuse that the narrative is nearly sunk by the weight of its scholarly anchor. The fact that most of the footnotes are either unnecessary or pedestrian further decreases the essay's intellectual buoyancy. Although these are problems of her own making, Hanne Eriksen, a doctoral candidate at Odense University, is relatively inexperienced in the writing of history. Someone in the house that also published Hans Chr. Johansen's demographic studies and Jørgen Hæstrup's multi-volume work on the Danish Resistance should have insisted that Eriksen give her essay a thorough revision and an expanded focus.

PETER VINTEN-JOHANSEN  
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HEINZ DUCHHARDT. *Protestantisches Kaisertum und Altes Reich: Die Diskussion über die Konfession des Kaisers in Politik, Publizistik und Staatsrecht*. (Veröffentlichungen des Instituts für Europäische Geschichte Mainz, number 87; Abteilung Universalgeschichte. Bei-

träge zur Sozial- und Verfassungsgeschichte des Alten Reiches, number 1.) Wiesbaden: Franz Steiner Verlag. 1977. Pp. viii, 384.

Heinz Duchhardt examines a particular kind of literary and political activity surrounding imperial elections from the Reformation to the dissolution of the Empire. Through a study of the pamphlet literature, correspondence, and administrative memoranda surrounding each election, he establishes the degree to which contemporary theorists and politicians thought about, and worked for or against, the possibility of a Protestant emperor.

A study of the imperial elections reveals an interesting kind of theoretical and, on occasion, diplomatic force within the Empire. In particular, Duchhardt's research makes clear that we can no longer assume that, aside from the occasional challenge by a Catholic rival from France or Bavaria, the Habsburgs remained more or less secure in their monopoly of the imperial throne. On the contrary, Duchhardt establishes that the Habsburgs had to—and did—feel themselves under constant pressure, pressure that arose initially from the houses of Oldenburg and Wettin and then, in the early seventeenth century, shifted to Frederick VI of the Palatinate and Gustavus Adolphus of Sweden. After Westphalia, it came out of the writings of political theorists such as Buxtorff, Moser, and Pütter. These lawyers abandoned the concept of an international, sacerdotal emperor in favor of an explicitly German monarch and argued that a Protestant emperor would serve as well as a Catholic one. Eighteenth-century cartoonists and pamphleteers took up the cause and with each election generated a flood of paper in its support.

In one regard, of course, the charting of this kind of activity constitutes a study in failure, if not in subjective history. For whatever the particular Catholic anxieties or the desire of individual pamphleteers and diplomats, the fact remains that throughout this entire period no Protestant prince occupied the imperial throne. Even more to the point, not one Protestant prince—either from within or without the boundaries of the Empire—ever declared himself an active candidate for that office. But Duchhardt demonstrates that, if ultimately they shied away from open declarations, certain of the princes and a surprisingly large number of their advisors undoubtedly gave careful consideration to the prospect of securing a Protestant emperor. Catholic diplomats, for their part, regarded such activities with alarm and shaped their imperial strategy from a perspective that was often distorted and overly fearful. In short, within both Catholic and Protestant camps, certain contemporaries held to the thought of a Protestant “takeover” and with

each imperial election participated in the rumors, plots, speculations, and debates that such a conviction spawned.

What is not always clear from Duchhardt's study is the relative impact of this activity upon the course of German history. Just how important, for example, in shaping Habsburg response to Frederick VI's election to the Bohemian throne was the contemplation of a possible Protestant emperor? Certainly there were other reasons, dynastic as well as confessional, that propelled Ferdinand to war. Indeed, one might well suspect that for Vienna the danger lay less in the prospect of a *Protestant* emperor than in that of a *non-Habsburg* emperor. In this regard, as Duchhardt moves from election to election, marshaling his evidence for activity in support of a Protestant emperor, he endows his data with an importance that must remain hypothetical without a more explicit linkage with the larger historical events.

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CHRISTOPH KLESSMANN. *Polnische Bergarbeiter im Ruhrgebiet, 1870-1945: Soziale Integration und national Subkultur einer Minderheit in der deutschen Industriegesellschaft*. (Kritische Studien zur Geschichtswissenschaft, number 30.) Göttingen: Vandenhoeck and Ruprecht. 1978. Pp. 306. DM 52.

In this thoroughly researched monograph, Christoph Klessmann (of the Ruhr University at Bochum) investigates the growth and surprisingly rapid assimilation of a sizable Polish population (about three hundred fifty thousand strong at its peak) in the Ruhr district of western Germany. Most of this story takes place between the 1890s, when the industrial upswing encouraged the first really large immigration from Prussia's Polish provinces, and the 1920s, when the inclusion of these provinces in the new Polish state and the simultaneously declining labor needs of the coal industry brought it to a halt. The general pattern here is familiar enough: new industrial concentrations drew their labor supply from increasingly distant agrarian regions, which in Germany's case happened to be partly Polish. Many of these workers intended to work in the mines only long enough to earn the money to buy some land back home; instead, most of them were assimilated, socially and nationally, by their new surroundings. True, there were efforts by Ruhr Poles to resist this process by establishing a national subculture in the west; Klessmann's principal analytical framework is the dichotomy between

various pressures for assimilation and opposition to it on the part of nationally conscious Poles. But the scales were heavily weighted in favor of the former; the Polish subculture in the Ruhr seems to have been essentially a short-lived holding pattern pending integration, the pace and conditions of which were not unlike those of European immigrants to the U.S. The issue of assimilation or maintenance of a diaspora national identity was brought effectively to a head by the re-establishment of Poland in 1919: when forced to choose between German and Polish citizenship, most Ruhr Poles went with their country of employment and were absorbed by their German surroundings during the next two decades.

Klessmann approaches this subject from a topical—"social scientific" rather than chronological-historical perspective. This seems unfortunate, for the phenomenon of the Ruhr Poles was decisively determined by a number of specific historical developments; treating it in nonchronological fashion robs this study of some of its heuristically most valuable connections. He refers briefly to a number of sociological theories, but the text itself is not excessively theoretical; in fact, he relies in positivist fashion on heavy doses of quantitative data to tell his story for him. As a result, although one must respect the thoroughness and conscientiousness of the research effort here, the work is not especially pleasurable to read. Like many younger German scholars, he writes in a sociological pidgin English, employing phrases like "intergenerationale individuelle vertikale Mobilität" and inflicting words like "signifikant" and "diffizil" on a language that has always done very well without them. And the presence of 1,100 footnotes (for 183 pages of text), many of them containing essential information and all located at the back of the book, effectively destroys reading continuity. As for substantive issues, one wonders whether the Polish nationalist subculture was mainly a response to the Ruhr environment, as Klessmann suggests, or a function of the changed attitudes that recent immigrants brought with them from the increasingly agitated Prussian east. Although the author distinguishes throughout between the Masurians of the Prussian East and other Poles (and persuades us that the distinction is valid), he does not do so with Upper Silesian Poles, who (apart from religion) probably had more in common with the Masurians than with other Poles. In general, Klessmann is very well versed in the history of the Ruhr district and its mining industries, but he seems less at home in East Prussia or Poland itself, relying here on a few older accounts. One especially notable omission is Lawrence Schofer's largely analogous work on the workers of Upper Silesia, *The Formation of a Modern Labor Force: Upper Silesia, 1865-1914* (1975), which would have provided

the basis for many interesting comparisons, dealing as it does with a Prussian-Polish mining society in an indigenous Polish rather than diaspora German setting.

The strength of this work lies in its analysis of Poles in the Ruhr rather than in its efforts to suggest explanations based on conditions in the Polish provinces. Klessmann is very careful in his use of sources (the most valuable of which are the records of the *Polenüberwachungsstelle* in Bochum and related police files): he keeps the reader informed of their many problematic aspects and is careful not to generalize beyond what the evidence will bear. He presents much that is new in detail, including thirty pages of tables drawn from German archives, and some that is new in interpretation as well; especially interesting is his account of the curiously tolerant, even patronizing, attitude of Hitler's government toward its Polish minority, at least until 1939. On the whole, this work is the most comprehensive study of the Ruhr Poles to date and recommended reading for students of Germany's mining economy as well as of its national problems.

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MARTIN KITCHEN. *The Political Economy of Germany, 1815-1914*. Montreal: McGill-Queen's University Press. 1978. Pp. 304. \$20.95.

Martin Kitchen, until now best known for his studies of the German military, here undertakes the ambitious task of sketching "the interaction between economic development and social structure" for nineteenth-century Germany in an effort to explain "why economic modernisation did not lead to social modernisation" (p. 7). Understandably, this excursion into the increasingly fashionable but very important and promising realm of political economy must lean heavily on the work of others, and the major contribution of this book lies in its skillful explication and synthesis of some of the most recent scholarship in the field. Kitchen relies heavily on the work of economic historians with a strong social history orientation—Wolfram Fischer, Hans Rosenberg, and Hans Mottek—and Kitchen's analysis is largely influenced by Fritz Fischer and his students, especially Helmut Böhme, and by Hans-Ulrich Wehler. This coupling of the Hamburg and Bielefeld schools of German history is a very natural one, and Kitchen's story will be quite familiar to those who have followed their work. Anyone seeking a brief, convenient, well-presented account of their findings concerning the importance of economics in Germany's unification under Prussia, the sociopolitical consequences of the "Great Depression" of

1873-95, the role of the Junker-industrialist alliance, and the part played by domestic politics and socioeconomic conditions in German imperialism can turn to this book with pleasure and profit.

The latest word is not, however, the last word, and Kitchen's book demonstrates the limitations as well as the promise of the historical analyses upon which he depends. To be sure, Kitchen is not entirely uncritical, and he recognizes that explanations couched in terms of the "Great Depression" or social imperialism are problematic. Yet Kitchen's study would have been more stimulating and convincing if he had ventured upon a critical engagement with the historiography instead of limiting himself to an unfootnoted narration of its findings. A more serious criticism is that political economy should be an enrichment of economic analysis, not an escape from it. The role of economic theory for political economy is virtually undiscoverable from this book, and it is difficult to see how Kitchen can deal with problems of economic modernization without seriously considering the theories of Alexander Gerschenkron or the work of David Landes. Their most important works do not even appear in Kitchen's bibliography. While Kitchen talks generally of the importance of the structure of German capitalism, he makes remarkably little use of the fundamental works on industrial structure by Rolf Wagenfuhr, W. C. Hoffmann, and Knut Borchardt, and he seems unaware of the very useful study on conflicts within the industrial sector by the East German historian, Helga Nussbaum. Thus, Kitchen talks about the coming of "monopoly capitalism" (p. 155) without ever defining what it is, and he seems totally at sea in dealing with the political implications of the conflicts between heavy industry and manufacturing industry before the war (p. 230).

Both the Hamburg and the Bielefeld schools have stressed the continuity in German history, and Kitchen shares this emphasis as well as the dangers that it runs. As Thomas Nipperdey has pointed out in a trenchant criticism of Wehler, the exclusive stress upon the power of conservative political forces in imperial Germany entirely neglects the evidence that might be presented in favor of a more open historical situation. It is important to note prefigurations of fascistic ideas, but it can be argued that Kitchen, like his historical mentors, buries the liberals before they are dead. Similarly, the early triumph of socialist revisionism is so strongly emphasized that one wonders what the revisionists had to revise. Finally, as the recent collection of essays—*Society and Politics in Wilhelmine Germany* (1978), written by young British historians of Germany and edited by Richard J. Evans—suggests, the politics of prewar Germany cannot simply be conceived in

terms of those above "manipulating" those from below and consists of a good deal more than the Junker-industrialist alliance. Kitchen's study of Germany's political economy would have gained from casting its net a bit wider in this respect as well.

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DAVID CALLEO. *The German Problem Reconsidered: Germany and the World Order, 1870 to the Present*. New York: Cambridge University Press. 1978. Pp. xi, 239. \$11.95.

In his well-written review of the German problem in international affairs, David Calleo warns us against treating Germany "as an isolated case" and urges us to understand rather than judge it. He then treats Germany as an isolated case and pronounces some harsh judgments, against its governments, at any rate.

The substance of Calleo's essay is distinguished by its able presentation of German rationalizations. But, in the end, understanding does not immobilize judgment. The author concedes that German assumptions of the inevitability of conflict decided the issue of peace and war in 1914 and 1939. He terminates the debate on page 115 with this devastating statement: "Hitler's Germany, like the kaiser's before him, was not strong enough for its ambitions." In the twentieth century German governments that pursued goals beyond their resources solved their difficulties by committing suicide. Calleo has therefore identified a problem and described its solution. But the book does not end on page 115. It continues with a sketchy chapter on the cultural roots of the German dilemma and concludes with a look at post-1945 Germany, which reveals that there exist no practical alternatives to the present order.

The precis of cultural history could have provided one of many opportunities for genuine international comparisons. Instead, even the summary of the German scene does scant justice to the fertility and variety of German prewar civilization. Cultural despair was not its only product. To judge the culture of Wilhelmine Germany by the levels of imperial statesmanship makes as much sense as judging *fin-de-siècle* France by the politics of the Dreyfus affair.

The book leaves the reader with the feeling that there is more to the German problem than two world wars. The reviewer suspects that this is precisely what Calleo intended. Still, one cannot help objecting that he has left the better part of his job undone. The quest for power has been suspended, the search for identity continues. The author con-

tributes little to the second problem, because his own German vision is so fragmentary. He wastes not a word on the monarchical system and the monarchical ideology, and he fails to grapple with the trauma of 1918, when the social and political traditions of both were uprooted. As a result, his conclusion that "no one did more to destroy traditional German society than Hitler" (p. 143) may be good Dahrendorf, but it is unendurably simplistic history.

Nowhere does this two-hundred-page reconsideration confront federalism and particularism, another gaping omission. Finally, the reviewer is bound to add that there exist today at least four Germanies, not two. Each presents us with a distinct political system, and this diversity represents a common denominator in German history more fundamental than the ephemeral works of Bismarck and Hitler and a phenomenon more persistent than the landmarks of modernization and industrialization. It is surely worthy of some note to discover a language whose products have enriched world civilization but to find several states and no nation to go with it.

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RUDOLF VON THADDEN, editor. *Die Krise des Liberalismus zwischen den Weltkriegen*. Göttingen: Vandenhoeck and Ruprecht. 1978. Pp. 277. DM 38.00.

This is a collection of fourteen papers originally delivered in the summer of 1976 at a German-Israeli conference on the crisis of German liberalism between the two world wars. The conference was organized along interdisciplinary lines and extended beyond history and political science to sociology, theology, philosophy, and literary criticism. Particularly illustrative in this respect are the essays by R. J. Zwi Werblowsky, Hans-Georg Geyer, and Nathan Rosenstreich on liberal, dialectical, and Jewish theology during the Weimar Republic, while Günther Patzig's examination of the irrationalist interpretation of Greek philosophy in the 1920s documents the shift in intellectual values that took place in Weimar Germany.

Of the individual contributions, only the two introductory papers by S. N. Eisenstadt and Martin Seliger try to place the crisis of Weimar liberalism in a broader historical and theoretical perspective. Less global in scope are the contributions by Rudolf von Thadden on the liberal deficit in German nationalism and conservatism, by Yehoshua Arieli on the crisis of liberal historiography in the nineteenth and twentieth centuries, by Rudolf Vierhaus on the notion of a "Deutscher Weg" in German social and political development, and by Werner Jochmann



on the liberal response to the rise of National Socialism. Of particular interest is Hans-Paul Bahrdt's incisive analysis of the methodological assumptions and shortcomings of Theodor Geiger's *Die soziale Schichtung des deutschen Volkes*, while Ernst Simon's essay on the crisis of German liberalism as reflected in the literary production of the Mann brothers, Robert Musil, Jakob Wassermann, and Arthur Schnitzler suffers from the absence of a unifying thesis. By contrast, Suitbert Ertel's attempt to distinguish between liberal and authoritarian styles of thought represents an imaginative excursion into the field of social semantics. Based largely upon the author's own experience during the Weimar Republic, the concluding essay by Gershom Sholem on the social psychology of German Jews from 1900 to 1930 is particularly rich in insight and general information.

Unfortunately, the papers are extremely uneven, and some are only tangentially related to the crisis of German liberalism in the interwar period. Moreover, the volume devotes precious little attention to the specific crisis of political liberalism during the Weimar Republic and, aside from Eisenstadt's highly abstract introductory essay, makes no attempt to examine the relationship between the fate of Weimar liberalism and the general crisis of liberal democracy in Europe following the end of World War I. These shortcomings severely limit the book's overall value as a contribution to the history of liberal ideas and institutions between the two world wars.

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WERNER SCHNEIDER. *Die Deutsche Demokratische Partei in der Weimarer Republik, 1924-1930*. Munich: Wilhelm Fink Verlag. 1978. Pp. 278. DM 48.

The German Democratic Party (DDP) and its leading personalities have attracted a good deal of attention in the last decade from historians who have been inclined to see in the fate of this party the key to understanding the fate of the Weimar Republic. The best-thumbed papers in the Bundesarchiv in Koblenz might be the Erich Koch-Weser collection. Koch was chairman of the party from 1924 to 1930 and twice a Reich minister. Almost every scholar who works on the political history of Weimar looks at Koch's papers. He is regarded as a central figure in the history of the DDP; and the DDP is regarded as central to the political history of the republic. Despite this attention, however, the party has yet to find its historian, or rather its historians' historian.

As Werner Schneider rightly points out, most of the attention devoted to the DDP has focused hitherto either on the early or later years of the republic or on individuals. Schneider consequently decided

to study the party in its middle years, 1924-30. The result is, ironically, probably the most systematic and useful analysis to date of the party as a whole. Using the by-now standard archival sources located primarily in Koblenz and building on the already extensive secondary literature, Schneider discusses in a workmanlike manner the organization and functioning of the party. Although he delivers some new information, on, for example, the structure and finances of the DDP and on certain policy matters, he nevertheless provides no dramatic revelations or even major reinterpretations. Schneider's strength is his prudence and sound judgment. Contrary to many of the historians of left liberalism, he has no axe to grind, and he does not indulge in wishful hypothesis. He demonstrates effectively how the tug of war in Weimar politics and the exigencies of social and economic crisis tore the DDP apart, literally limb from limb, and how the torso of the party, whose ethos was moderation, reconciliation, and a sense of responsibility, came increasingly to resemble the ass in Buridan's fable. Rather than becoming a party of synthesis, as it hoped, the DDP became a party of contradictions, incapable of attracting new support and incapable of retaining the support it had. Its natural inclination was to protect the status quo, and this was, of course, a disastrous position to take in the midst of Weimar's travail.

The attempt to combine a thematic and chronological approach makes Schneider's discussion jerky and repetitious at times. Also, he lapses occasionally into an irritating jargon. But, all in all, this is a fine study.

MODRIS EKSTEINS  
University of Toronto

FELIX HIRSCH. *Stresemann: Ein Lebensbild*. Göttingen: Musterschmidt. 1978. Pp. 335. DM 48.

Since Gustav Stresemann's extensive private papers and the voluminous documents of the interwar German foreign ministry became accessible to scholars in the mid-1950s, numerous aspects of his career have been analyzed closely in an impressive array of scholarly books and articles. Yet, as a quarter century of intensive research drew to a close, no full-scale biography assessing his life as a whole and establishing the relationships between its variegated and sometimes seemingly contradictory chapters had appeared.

After many years of promising to remedy this situation, Felix Hirsch, who emigrated from the Germany of the thirties to this country, has now delivered the results of his labors. In this *Lebensbild* he expands, but does not fundamentally alter, the views he set forth in a brief sketch of Stresemann's life issued by the same publisher in 1964, the sub-



title of which reveals much about his interpretation: *Stresemann, Patriot und Europäer*. Ignoring or bagatellizing the now abundant evidence that Stresemann, as foreign minister of the Weimar Republic from 1923 until his death in 1929, was always first and foremost a German nationalist whose paramount aim was to unwind the "chains of Versailles," Hirsch again portrays him as a forerunner of the post-World War II European unity movement. As a consequence, in the portions of the book dealing with Stresemann's foreign policy, the tone is that of a defensive apologia. Hirsch rarely attempts to counter the well-documented analyses that contradict his interpretation. Instead, he dismisses his unnamed scholarly adversaries as *gehässige Kritiker* (some ominously alluded to as "foreigners") who have sought out of sensationalist motives to sully the reputation of a great statesman. One seeks here in vain for an informed dialogue with the now abundant scholarly literature on Stresemann.

Nor does the reader find much information on Stresemann's policies as foreign minister. Declaring apodictically that the "hair-splitting" of diplomatic historians has no place in a *Lebensbild*, Hirsch recounts Stresemann's laborious pilgrimage from conference to conference, dwelling at length upon his personal relations with his associates and the diplomats of other countries and also upon his family life, his artistic tastes, his physical ailments, and his unquestioned strength of will. His account of Stresemann's career as foreign minister is unbrokeably adulatory, protective, and—toward the end of his life—pathos laden. It is also heavily larded with nice things said about Stresemann by famous people, for the most part after his death. What occurred substantively before, during, and after important negotiations is alluded to only in cursory fashion. A reader seeking even a basic understanding of Stresemann's foreign-policy aims and the strategy and tactics he employed in the pursuit of these will thus have to look elsewhere. As Hirsch repeatedly emphasizes, it is the *man* who interests him, not the politician, the chancellor or the foreign minister. Unfortunately, the information Hirsch marshals on Stresemann will predictably fail—for all except the man's most uncritical admirers—to dispel the widely held view that, aside from his political career, he was a singularly unremarkable person. And most readers will find that Hirsch has succeeded only in making what was actually a highly interesting career seem banal.

Even more curious than Hirsch's conception of what a biography should comprise is his attitude toward evidence. Although Stresemann's papers have been widely available on microfilm for twenty-five years, Hirsch seems to have dipped into them in only cursory fashion. One finds only a few

very imprecise footnote references to this rich store of documents in the chapters on Stresemann's career down to the early 1920s. Thereafter, even these disappear. There is no indication that the foreign ministry papers, also widely available in microfilm copies, were consulted at all. Instead of drawing upon these indispensable sources, Hirsch falls back upon the *Vermächtnis*, a notoriously untrustworthy edition of documents selected from Stresemann's papers by his private secretary and published in the early 1930s. In his concluding chapter, Hirsch is quite explicit about his scorn for those historians who write about Stresemann solely on the basis of documentary evidence. They can never pretend, he assures his readers, to the sort of insightful knowledge about the man gained by those fortunate enough to come into personal contact with him, as Hirsch himself did as a young journalist in Berlin. So much for history written "aus den Akten"—and for virtually all historians.

In sum, a worthy biography of Stresemann is still to be written.

H. A. TURNER, JR.  
Yale University

KEITH W. BIRD. *Weimar, the German Naval Officer Corps, and the Rise of National Socialism*. Amsterdam: B. R. Grüner; distributed by Humanities Press, Atlantic Highlands, N.J. 1977. Pp. ix, 313. Paper \$28.25.

This book provides a useful introduction to the history of the navy in the Weimar Republic, a subject that has been largely overlooked by historians. To many the navy was seen as the prime cause of the republic, for it was argued that the naval mutinies had triggered off the "revolution" of November 1918. Thus, from the outset, the navy was the center of an intense political controversy. Unlike the army, the navy had few great victories to celebrate, and even the "Victory of the Skagerrak" was often found within inverted commas. The "stab-in-the-back" theory did not help very much, for the navy had to admit that its wartime officers had failed and that it was impossible to blame everything on outsiders.

The navy's leaders were determined to give the new navy a sense of mission, but it was difficult to see what that mission could be. Talk of "seapower" (*Seegeltung*) could mean almost anything and was usually an excuse for failing to think seriously about the role of the navy in the new state. This failure served to increase the sense of frustration. Things came to a head during the Kapp putsch, which was supported by Admiral Trotha and most of the naval officers, who staged a remarkable strike against an attempt by the petty officers and men to create a genuinely republican navy—a fascinating

episode about which we are given insufficient details. After the failure of the putsch many of Captain Ehrhardt's men continued to serve in the navy, frequently taking orders from him rather than from the proper authorities. Such radical antirepublicanism was particularly strong among the officer cadets, who ostentatiously celebrated the murders of Erzberger and Rathenau.

Predictably, many people were upset at this behavior. Civilians objected to the celebrations of the Kaiser's birthday, the flying of flags at half-mast on November 9, and the wearing of swastika badges on naval uniforms. But the authorities failed to take a firm stand against these activities. There was no proper control from Berlin of what was going on in the ports. The responsible minister, Gessler, was singularly ineffectual. Even Ebert was unable to stop the appointment of a prominent sympathizer with Kapp as captain of the cruiser "Berlin." Under these circumstances it is hardly surprising that the revived navy league (BDM), with its twenty-six thousand members, was a mere shadow of Tirpitz's organization.

The BDM was not the only element of continuity with the imperial navy. The same tired clichés and slogans were used, the same code of honor, the emphasis on religion echoing the old "throne and altar" alliance, and the idea of the navy as the "school of the nation," not of the republic but of some idealized vision of the German state. Thus, Raeder could be blatantly antirepublican and still claim to be "unpolitical." Even National Socialism, which most of the officers heartily endorsed, was not exactly what they wanted. Hitler was not William II, he announced, and he was sharply critical of Tirpitz in *Mein Kampf*. Clearly, in the Third Reich, the navy would never regain the prestige it had enjoyed in the Tirpitz era, and when the war began Raeder wrote that the best the navy could hope for was to "die gallantly."

Although the book contains much useful material, some critical issues are treated somewhat perfunctorily. The "stab-in-the-back" issue is not very well done, and Tirpitz's dramatic role almost overlooked. More details of the Erhardt brigade would be useful. The "pacifism" of the SPD deserves comment and drastic qualification. More details of Schleicher's intrigues that involved the navy would be of considerable interest. A clear account of the organizational structure of the navy would be helpful.

The book is also in need of serious editing. There is too much repetition, even of quotes, which is particularly noticeable in the third section. The author has been reading so much German naval prose that we are treated to such oddities as "one has had the nose full" and "an elephant in a porcelain shop."

There are an alarming number of printer's errors, and the bibliography is incomplete. An index would also have been useful. This is, nonetheless, a worthwhile book and required reading for anyone who still insists that the navy was somehow more liberal and "better" than the army.

MARTIN KITCHEN  
Simon Fraser University

KLAUS SCHAAP. *Die Endphase der Weimarer Republik im Freistaat Oldenburg, 1928-1933*. (Beiträge zur Geschichte des Parlamentarismus und der Politischen Parteien, number 61.) Düsseldorf: Droste Verlag. 1978. Pp. 313. DM 72.

Oldenburg represents an obvious subject for a case study both of the rise of Nazism and of the Nazi party in power: it was the first German state in which the Nazis won an absolute majority and the first to form an exclusively Nazi government. Klaus Schaap has now provided us with such a study and, as far as it goes, it is a solid piece of research. It presents a thorough account of the politics of the state during the years 1928-32.

The author brings out clearly some of the problems of Oldenburg: its constitutional vulnerability to plans for reform of the federal structure; the economic vulnerability of its pig and cattle farmers, who were discriminated against by the Reich government's tariff policy that operated in favor of East Elbian landowners and of industry; and, finally, the particular difficulty of achieving a coalition among the prorepublican parties such as existed in Prussia, given that the Center Party in Oldenburg represented the interests of the rural population in the Catholic districts of the south, while the SPD represented urban working-class consumers. He shows the ways in which the Nazis exploited the growing economic discontent of the rural population and of middle-class urban groups and charts their increasing political alienation by analyzing the various elections between 1928 and 1933. Finally, he provides a useful account of the period of Nazi government in Oldenburg after June 1932, illuminating its brutality and incompetence—characteristics that resulted in a massive 22.3 percent loss of support for the party in the November 1932 Reichstag election.

Yet, despite its considerable merits, the book came as a slight disappointment to this reviewer. One of the advantages of local history over the study of high politics at the national level is that it can provide a sense of concreteness and immediacy—of the peculiar flavor of a particular area and its people, getting closer to what was actually happening on the ground and thereby, in turn, providing a richer explanation of what was happening higher up. Yet, there is little of this in Schaap's

book. He provides no historical background—the book begins more or less abruptly in the mid-1920s—and his analysis of the social, economic, and denominational structure of the state is limited to two or three pages of what is mainly statistical material. This is useful as far as it goes, but it is no substitute for a sensitive elucidation of what made Oldenburg politics tick. There is virtually no analysis of the local parties and their leaders; indeed, Schaap does not appear to be interested in people—a grave disadvantage for a local historian. His comments on the often colorful figures of Oldenburg politics during this period are almost entirely confined to brief biographical footnotes; and, although we are all aware of the limits of the interview as a research tool, this does not entirely explain the author's apparent failure to interview a single participant in these events. The result of these weaknesses is that we learn less than we might otherwise have done. As far as issues of national importance are concerned, the book largely confirms what we already knew. If *Landesgeschichte* is going to provide us with important new insights, it must adopt a different perspective—rather than simply reproducing the study of high politics at a lower level.

JEREMY NOAKES  
University of Exeter

H. A. TURNER, JR., editor. *Hitler aus nächster Nähe: Aufzeichnungen eines Vertrauten, 1929–1932*. Frankfurt/Main: Ullstein. 1978. Pp. xvii, 508. DM 48.

This is a remarkable book. Lucid, candid, and discursive, it is ultimately a monument of instructive inadvertence. The book recalls the years of Nazi breakthrough with singular immediacy. It also recalls what its author, Maj. Gen. (ret.) Dr. h.c. Otto Wagener, a bona fide if short-lived Nazi insider, clearly considered his finest hours. Even the history of the manuscript has its documentary interest. Committed to paper, and lots of it, in a British POW camp, Wagener's text beat its author home from postwar Italian imprisonment by some five years. In 1957 it was acquired by the Munich Institut für Zeitgeschichte for an undisclosed price. Skillfully edited and mercifully abridged by Henry A. Turner, Jr., it is finally accessible, to the library reader at least, with a handsome complement of contemporary photos.

Nice as it might have been to have in 1932, 1942, or 1952, Wagener's relentless chronicle has lost little of its intrinsic fascination. Between 1929, when he came for an SA parade and moved in, and 1932, when he left for Berlin and was effectively moved out, Wagener was there like a Brown House Boswell, purposefully noting what was said, thought, and done. There is nothing else quite like it—not Rauschning, a shrewd and credible but inevitably

limited visitor from outside; not Speer, whose first-person saga reflects drastically different times and circumstances; not Krebs, the Hamburg Gauleiter, whose story is fundamentally his own as seen from the periphery.

Wagener's entries describe the years when the Nazis came out of their rented walk-ups into the light, when names and money started taking out insurance, and when fat rats started coming aboard. Big cars, fast trains, good hotels, and long-distance phone calls appear with impressive and increasing regularity. So do new contacts, connections, exploratory conversations, schemes, dreams, cabals, and an accelerating competition for the leader's ear.

Hitler, not surprisingly, is one of the book's major figures: windy, cunning, fanatic, pretentious, and predatory, despite Wagener's sporadic efforts to deodorize his memory. There are occasional unaccustomed dimensions like a fixation, real or disingenuous, on early Christianity. But the familiar features are all here—Hitler the spellbinder, the pill-popper, the vegetarian, the celibate, the insomniac, the lion, and the fox. Students of Nazi foreign policy will find corroborations of Rauschning. Students of Hitler will find corroborations of *Mein Kampf* and the wartime tabletalk. For students of the Third Reich there are plausible anticipations of the regime in power from its architectural fantasies to political boarding schools.

But Wagener himself is the book's real discovery. Naive, assiduous, self-righteous, and dumbly patriotic, a prodigy of self-pity and an unremitting sucker for flattery, the author is Mr. Middle-Class German, the inevitable link between the cloddish condottieri who preceded him and the bloody careerists who were to crowd him aside. Wagener is the Nazi idealist in full flower. A jowly Parsifal and self-appointed keeper of the "socialist" grail, determined to swallow his own creeping reservations, he has a mind like an old Hearst Sunday paper. Others testify in their way to the banality of evil. Wagener's memoir testifies like none before it to the potential evil of banality. Its publication is a real service.

DAVID SCHOENBAUM  
University of Iowa

DAVID IRVING. *The War Path: Hitler's Germany, 1933–1939*. New York: Viking Press. 1978. Pp. xvii, 301. \$14.95.

David Irving's books on Hitler serve as vivid illustration of the truism that the predilections of the historian may determine the uses he makes of his evidence. The present volume, which completes Irving's study of Hitler by filling in the prewar years, re-emphasizes the highly personalized view of the German dictator that he set forth in *Hitler's War*.

Convinced that previous writers have maligned and misunderstood the German dictator, he is determined to "de-demonize" Hitler by showing that he was neither a "madman" nor the "Devil incarnate." Actually, we are told, he was an average sort of person: "an ordinary, walking, talking, human weighing some 155 pounds" who would sometimes "wrinkle his forehead and put out his tongue."

Irving has diligently sought new evidence to support his singular interpretation, and in truth he has found materials not previously known to Hitler scholars: Heinrich Himmler's personal telephone log, some private papers of Ernst von Weizsäcker, letters of Hitler's youthful admirer and later adjutant, Walther Hewel, and unpublished diaries of SS and Luftwaffe adjutants. The author assures us that the original manuscript of the book contained "1500 pages of source notes." Regrettably, owing to publication costs, he is able to show us only representative samples in endnotes that often invite the reader to go to Munich to consult the "Irving Collection" deposited in the Institut für Zeitgeschichte. He must be given credit for ferreting out these sources. Less credit is due for the way he uses his own documents and dismisses evidence of proven validity that refutes his contentions. His distrust of published documentation and of academic historians is profound.

Given the author's predilections and his selective use of evidence, a new picture of the German dictator does indeed emerge. Irving's Hitler was a kindly man, "much given to unpublished acts of humanity." Sexually—as in other respects—he was completely normal, but, heroically "casting off all fleshly pleasures," he dedicated himself to serving his country. And what prodigious services he performed! In a few short years he "restored Germany's psychological unity," "abolished class warfare," "created a Germany of equal opportunity," and was responsible generally for "strengthening the character of his people." Irving can find only one major flaw in the "restorer" of Germany: he was gullible. Time after time this well-meaning man was duped and deceived: by Himmler, by Goebbels, by the Gestapo, by the army, by the SS. It is Irving's thesis that because Hitler was "deeply in thrall of his devious henchmen," major decisions in the Third Reich were not made by the Führer at all.

In the blood purge of 1934, for example, Hitler was "unwittingly duped by the Army and the SS." When he found out about the murders, he was so "unsettled" that he could not sleep at night. With typical magnanimity, according to Irving, he ordered that state pensions be given to families of the victims—exactly as he would pay pensions to the next-of-kin of the participants in the Bomb Plot of 1944. A reader seeking evidence for these remark-

able assertions is referred to "Julius Schaub's private papers" to be found in the *Sammlung Irving*. (Schaub was the dull-witted sycophant who served as his Führer's personal adjutant.)

Similarly, Hitler was not responsible for the pogrom of November 9, 1938, the infamous *Reichskristallnacht*. That was an "arbitrary act" of Goebbels, which hit Hitler "out of the blue." When he learned of it. He was furious and immediately gave orders to protect Jewish shops from arson and looting.

Irving maintains in *Hitler's War* that historians have also been deceived about Hitler's role in the Holocaust: he had not ordered the genocide and did not even know it was taking place until many months after it had started. Indeed, Hitler actually gave an explicit order in 1941 that there be "no liquidation" of the Jews. Yet, in spite of that order, his overzealous satraps "pulled the wool over Hitler's eyes" and carried out the genocide against his wishes. Since Irving sweeps aside all the solidly established testimony connecting Hitler directly to the Final Solution and rests his case upon a bit of his own evidence and since his use of that evidence nicely illustrates his methodology, it must be cited. It consists of a note that Himmler apparently jotted down after a conversation with Hitler, dated November 30, 1941. It reads in full, "Judentransport aus Berlin, Keine Liquidierung." In discussing this evidence, Irving does not quote the important first line referring to one particular shipment of Jews. Instead, he flourishes the two words, "no liquidation," as if he were executing a *coup de maître*. No less than six times he asserts that these two words substantiate his claim that Hitler gave blanket orders forbidding the genocide. His own evidence does not warrant that conclusion; a mountain of testimony refutes it. Irving's assertion that Hitler personally wished Polish priests and intellectuals no harm, that he merely wanted them "accommodated elsewhere," must be regarded as a very sick joke.

The author's private archives can apparently cast no fresh light on other issues of the Third Reich. Consequently, he uses his "unpublished sources" to regale the reader with details of marital conflict and scandals among Hitler's lesser adjutants and their wives. The subtitle of the book is not justified. One will find here no discussion of the social, cultural, or institutional history of Germany from 1933 to 1939; little is added to our knowledge—and nothing to our understanding—of Hitler's foreign policy.

It is unlikely that this book will enhance Irving's reputation among serious historians.

ROBERT G. L. WAITE  
Williams College

GERD R. UEBERSCHÄR. *Hitler und Finnland, 1939-1941: Die deutsch-finnischen Beziehungen während des Hitler-*



*Stalin-Paktes.* (Frankfurter Historische Abhandlungen, number 16.) Wiesbaden: Franz Steiner Verlag. 1978. Pp. ix, 372. DM 78.

With the current interest in the question of "Finlandization" and the unique position Finland has as the only non-Communist Western European state bordering the Soviet Union, it is a genuine pleasure to see the publication of this work. Finland's role in World War II has been a relatively neglected area of study and remains an enigma to all but a few. Although some studies have appeared that deal with specific elements of the Finnish problem, for example, the Petsamo question and the diplomacy of the Winter War, no comprehensive work has been published until now covering the crucial period from 1939 to 1941, when the fateful decisions that brought Finland into the war with Germany against Russia were made.

Originally written as a dissertation, the work is a thoroughly and deeply researched study. Beginning with a brief résumé of Finnish history from independence to the onset of its difficulties with the Soviet Union in 1938, Gerd R. Ueberschär lucidly and succinctly fits Finland into the historical, geopolitical, military, and social context of Europe in the interwar period as well as its unique position vis-à-vis Scandinavia, Germany, and the Soviet Union.

Basing his analysis not only on diplomatic but also on military and naval documents, Ueberschär has drawn a concise and clear picture of Finland's agonizing position between Germany and Russia. While attempting to remain faithful to Nordic neutrality, the world situation perforce drew Finland into the fateful struggle between the Great Powers. Although Hitler evinced little or no interest in Finland politically or militarily up until the Winter War, that event was the turning point in Finland's destiny. From then on, Finland, like Rumania, became a necessary cog in the Nazi war machine.

Throughout the book Ueberschär effectively portrays the efforts of the Finnish government to avoid involvement in the war, but he also elucidates the inherent pro-German attitudes among influential Finns that underlay the decision-making process. He also brings into focus the traditional distrust and hostility toward Russia inherent in the Finnish people. Together with its geopolitical position these interests and attitudes brought Finland into a war that it could not hope to win without German assistance. When Hitler made that aid readily available, Finland's role and fate in World War II were sealed.

Though primarily concerned with diplomacy, Ueberschär does not neglect the economic aspects of Finnish-German relations. He adroitly shows the relationship of the Petsamo nickel question to the

larger question of Finland's strategic and political importance to both Germany and the USSR in the aftermath of the Winter War.

For anyone who would be a serious student of the area and the time, this book is a necessity. It is a treasure-trove of bibliographical, literary, and documentary sources requisite to any study of Finland's role in World War II.

RICHARD W. CONDON  
Mansfield State College

HONORÉ M. CATUDAL, JR. *A Balance Sheet of the Quadripartite Agreement on Berlin: Evaluation and Documentation.* Foreword by KENNETH RUSH. (Political Studies, number 13.) Berlin: Berlin Verlag. 1978. Pp. 303. DM 32.

With this book Honoré M. Catudal, Jr., completes his two-volume study of the Soviet-U.S.-British-French Agreement on Berlin of September 3, 1971. In the first volume (reviewed, *AHR*, 83 [1978]:759-60) he traced the diplomatic negotiations that led to the signing of the agreement and its attendant inner-German agreements and provided some of the long-range historical background. In the present volume he carries out a systematic and detailed survey of the major policy fields in which the Quadripartite Agreement has effected changes in the status and viability of West Berlin.

The overall balance, he finds, is positive (from the Western point of view): "undeniable change for the good has occurred as a result of the Quadripartite Agreement" (p. 137). This situation, he convincingly argues, is the result in large part of reluctant but genuine concessions by the Soviets to the Western position. Catudal explains the shift in the Soviet stand as one motivated by considerations that transcend the local context: a desire for increased trade with the West, including Western Germany, as a means of acquiring advanced Western technology; a desire to strengthen détente; and concern over the smoldering conflict with Communist China. In Catudal's view the Soviet willingness to make concessions on Berlin has not, however, fundamentally altered the long-term Soviet and East German goal of isolating West Berlin from West Germany and ultimately undermining its ability to survive as a politically and economically viable entity. Thus, while the Berlin problem is no longer the source of recurring international crises, it continues to serve as "a seismograph of political change . . . reflecting the ups and down of a shaky détente" (p. 133).

The volume is rounded out by a chronology of events, texts of some of the earlier diplomatic instruments that define the status of Berlin, the text of



the Quadripartite Agreement itself and the principal associated inner-German agreements, a series of statistical tables on the economic position of West Berlin in the postagreement period, and a bibliography. Taken as a whole, Catudal's two-volume study is an example of contemporary history at its best and deserves the attention of anyone concerned with recent international relations and the current strategic balance between the Soviets and the West.

ROBERT M. SLUSSER  
Michigan State University

HERMANN WIESFLECKER. *Kaiser Maximilian I.: Das Reich, Österreich und Europa an der Wende zur Neuzeit. Volume 3, Auf der Höhe des Lebens, 1500–1508; Der Grosse Systemwechsel; Politischer Wiederaufstieg.* Munich: R. Oldenbourg Verlag. 1977. Pp. xxviii, 622. DM 98.

For the Graz historian Hermann Wiesflecker the publication of this book represents another major step toward the completion of an ambitious project that has been thirty years in the making. Since 1948 he has produced a steady stream of meticulously researched articles on a wide variety of questions and themes related to the reign of Emperor Maximilian I. The cumulative results of these articles guided him naturally into his current project: a comprehensive, five-part biography on which he has been working for the past decade. In the first volume, which appeared in 1971, Wiesflecker examined Maximilian's youth and early career prior to the death of his father, Frederick III. In the second, finished four years later, Wiesflecker extended his treatment of the embattled Habsburg's life through the Diet of Augsburg in 1500 (*AHR*, 82 [1978]: 659). Now, in volume three, he has traced the recovery of Maximilian's personal fortunes in Germany and Europe from their nadir at the turn of the sixteenth century to what he views as their high point following the Diet of Constance (1507). During this tumultuous period, continuing opposition from Venice prevented the king of the Romans (a title that Maximilian had acquired in 1484) from traveling to Italy for an official imperial coronation. He nonetheless managed, despite this handicap, to implement extensive financial and administrative reforms within his hereditary domains, survive a debilitating constitutional struggle with the German princes, achieve a decisive victory in a Bavarian-Palatinate war of succession, and initiate marriage negotiations with the Jagellon dynasty that ultimately proved crucial in the founding of the Danubian monarchy.

Wiesflecker has examined each of these problems at length, drawing upon an impressive array of ar-

chival sources, published documents, and secondary authorities to support essentially favorable assessments of Maximilian's actions and ideas. Although he takes pains to defend his own objectivity, Wiesflecker cannot hide his obvious admiration for Maximilian, whom he considers a skilled politician guided in all of his undertakings by deeply rooted religious and metaphysical concepts, above all by a sense of high mission as God's chosen instrument in the affairs of men. Most readers will feel comfortable with Wiesflecker's sympathetic portrait and will certainly admire his dedicated scholarship. Yet some may regret that he has confined himself largely to an old-fashioned narrative of political, diplomatic, and military events. Perhaps in his final volume, in which he promises to discuss the economic, social, and cultural ramifications of his subject, Wiesflecker will move beyond a conventional framework and offer conclusions that will help us to understand Maximilian and his world from a more innovative perspective.

JOHN A. MEARS  
Southern Methodist University

ERNST WANGERMANN. *Aufklärung und staatsbürgerliche Erziehung: Gottfried van Swieten als Reformator des österreichischen Unterrichtswesens 1781–1791.* Vienna: Verlag für Geschichte und Politik. 1978. Pp. 126. S 148.

In this brief monograph, Ernst Wangermann has attempted to rescue the younger Gottfried van Swieten from the reputation for zealotry and mechanistic rationalism that has hung over his figure for a century. The son of a justly famous father, van Swieten in several ways epitomizes the Austrian Enlightenment. In examining his career as the chairman of the *Studienhofkommission* in the 1780s, Wangermann also makes a solid contribution to the ancient discussion over the nature and goals of Josephinism.

The importance of van Swieten's post in the eyes of the emperor can scarcely be exaggerated. Joseph II was convinced, as he repeatedly stated, that the creation of a mentally and physically productive citizenry was the premier task of government. His own didactic instincts were irrepressible and the plague of many a subordinate. But Joseph was also the chief executive of an inherently centrifugal construct called the Habsburg dominions. Throughout his short reign the emperor entrusted to van Swieten the task of finding the balance between the desired *aufgeklärte Gesinnung* and the necessary *Gehorsam*.

Wangermann shows that van Swieten's reputation as a pedant and slave to paper schemata is

much overdone, if not deliberately distorted by a later generation. It was not his pedantry but the latent conflict between an informed and an obedient population as the first priority of Joseph's educational policy that haunted all his efforts and eventually brought him down. The source of most of his troubles was predictable: the powerful and resentful clergy headed by the redoubtable Cardinal Migazzi. Van Swieten's enthusiasm for the General Seminaries controlled by the state and his insistence that the education of priests be conditioned by their education as enlightened citizens ensured his status as *persona non grata* with the Church. The battle was joined over a series of relatively innocuous matters and still hung in the balance at the time of Joseph's death. But the tide was clearly moving away from van Swieten as the news from revolutionary France grew ever more disquieting; his removal from office by Joseph's successor in 1791 was no surprise.

It becomes clear in Wangermann's pages that the pedagogical and philosophical differences between radical and moderate *Aufklärer* in Austria were relatively insignificant; rather it was the conundrum of reconciling the ideal of the enlightened and productive subject with the necessity of order and *Ruhe* within the realm which separated van Swieten from his bureaucratic opponents. In the end, it boiled down to who could more successfully prognosticate which of the emperor's controlling impulses would emerge on top: the desire to renew humanity or the desire to assert the power of the house of Habsburg. Van Swieten consistently opted for the cause of humanity, as he understood it; in so doing, he not only became the target of the conservatives but also tended to forget the exigencies of power. The glories of the *schöne Wissenschaften*, which van Swieten so fervently promoted in his projects for university reform, were lost upon an emperor increasingly beleaguered by the rumblings of internal revolt and external war. What Joseph needed after 1786 were good soldiers more than enlightened citizens; faithful bureaucrats more than autonomous thinkers. The Church and the conservative faction promised to deliver them, while van Swieten and his handful of allies in the *Hofkommission* could only protest that in the long run the empire would have to have both to survive.

Wangermann is eminently successful in showing how a generation of *parti pris* historians have twisted van Swieten's work and his pedagogical reputation. The monograph is held within very narrow bounds, however, and it would have been useful to a wider circle of readers to have had somewhat more discussion of the Josephinist context in which the *Hofkommission* and its leader worked and struggled. The author's sympathies with his protagonist are clear and forthright and must be shared to a degree by all historians who read the moving defense of their

discipline (p. 106) written by van Swieten as the official curtains began to close over him. He was, as Wangermann states, "in tune with Reason, but opposed by the Times." In such affairs, there can be little doubt as to the eventual victor.

PHILIP J. ADLER  
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HANS BERGER. *Der Alte Zürichkrieg im Rahmen der europäischen Politik: Ein Beitrag zur "Aussenpolitik" Zürichs in der ersten Hälfte des 15. Jahrhunderts.* Zurich: Verlag Hans Rohr. 1978. Pp. v, 232. 30 FR.

Hans Berger has written a book that will quickly enter into the unenviable status of a valuable but seldom read work, for its insights are presented in the form of traditional diplomatic history at its most tedious. Reading the book is hard work made no easier by the absence of both an index and desperately needed maps. All of these shortcomings are unfortunate because Berger provides an up-to-date reconstruction of one of the most exciting events in Switzerland's heroic age: *Der Alte Zürichkrieg*. This war between Zurich and Schwyz over the Toggenburg inheritance is properly remembered as a turning point in Swiss history; its outcome broke Habsburg revanchism in the area, ended the growth of a powerful Zurich state outside of the Swiss Confederation, and brought that confederation into military and diplomatic contact with Valois France. This conflict has been traditionally characterized as Switzerland's great "civil war," and it was Berger's intention to demonstrate the limitations of using such a term, with its nationalistic assumptions, in the study of these events.

Berger's story begins in 1415 when the occupation of Aargau by the confederation eliminated the Austrian presence in the area, leaving the Habsburgs only pretensions and hopes for restoration. The diplomatic situation of the time was incredibly complex. The several Swiss territories still pursued independent and conflicting policies in a milieu in which the Habsburgs were not yet emperors and imperial princes and cities, Savoy, the Church, the Valois, and Burgundy were all powerful separate forces to be reckoned with. Berger characterizes at great length Zurich's status as an imperial free city and describes the evolution of its ambitious diplomacy over the next twenty years as its dealings with the emperor, the princes, the cities, the Church, and the confederation were grounded on the consistent policy of territorial expansion. This policy collapsed in the years after 1436 when Zurich's seizure of the Toggenburg lands led it into bitter conflict with Schwyz. By 1446 Zurich was isolated from the confederation and militarily defeated. In pursuit of the bankrupt policy, Zurich found itself driven into an

alliance with the traditional Austrian enemy, once the Habsburgs had assumed the imperial title, and dependent for military support on the dubious but destructive support of the dreaded French mercenaries after 1444. The war and ruin of Zurich ironically strengthened the Swiss Confederation; thereafter, Zurich was an integral part of the emerging Swiss state. Civil war and the intervention of the great powers may have destroyed sixteenth-century Italy, but they helped force the creation of fifteenth-century Switzerland.

The question remains whether such a tendentious diplomatic history, however well researched in archives, chronicles, and secondary literature, can resolve a question of political values. One might hope that the interesting question, among many others, of the "civil" character of this war will be followed up as promised in further work. What is offered here is an interesting theory incompletely developed but presented in a suggestive perspective. Whether or not Zurich has been exculpated from the charge of treason against the confederation, Berger has succeeded in drawing a coherent picture of fifteenth-century Alpine politics and in reminding us that that milieu must be understood in its own terms. Berger has effectively demonstrated the limitations of the historiographic tradition he sought to criticize. Henceforth, Alpine politics in this era will be considered not only in terms of the emergence of modern Switzerland but also "im Rahmen der europäischen Politik."

PAUL SOLON  
Macalester College

RUDOLF VETTERLI. *Industriearbeit, Arbeiterbewusstsein, und gewerkschaftliche Organisation, dargestellt am Beispiel der Georg Fischer AG (1890-1930)*. (Kritische Studien zur Geschichtswissenschaft, number 28.) Göttingen: Vandenhoeck and Ruprecht. 1978. Pp. 344. DM 78.

Swiss industrialization has attracted little attention from historians because it generally appears to parallel the course followed by its larger neighbors. Yet, as Rudolf Vetterli's case study of the Georg Fischer steel mills in the German-speaking town of Schaffhausen demonstrates, Switzerland did not always face the same difficulties in adjusting to industrial production as did Germany and other European powers. Using company and trade union archives and borrowing an analytical model developed by German historian Jürgen Kocka in *Unternehmensverwaltung und Angestelltenschaft am Beispiel Siemens 1847-1914* (1969), the author examines the impact of changing social and industrial conditions in the Fischer works on the outlook and organizational behavior of Schaffhausen's metal workers between 1890 and 1930.

The most important contribution made by this book is its perceptive analysis of how the organization of steel production in Schaffhausen produced social and psychological divisions between the skilled and unskilled worker, the Swiss and foreign worker, and the town dweller and rural commuter that were exploited by the Fischer management to neutralize local trade unions. These same conditions also prevented development of the strong working-class identity and organizational unity needed to develop an effective trade union movement. In calling attention to the critical role played by factory conditions in the development of labor attitudes and behavior, a factor sometimes overlooked by historians unfamiliar with the industrial process, Vetterli has provided labor historians with a useful analytical framework. No traditional analysis of industrial conditions in Schaffhausen, whether from the perspective of the company boardroom or the trade union office, could have explained so thoroughly the reasons for labor's impotence in the Fischer works before 1914.

Vetterli's approach is less successful, however, in accounting for the radicalization of industrial relations in this northern border town between 1914 and 1930. Deteriorating factory conditions during the latter stages of World War I may have helped to intensify worker dissatisfaction, but these factors alone did not produce the sudden upsurge of labor demands in 1918 or a communist-dominated metal workers union in Schaffhausen during the 1920s. These developments were clearly the result of political and international factors outside the walls of the Fischer concern. Thus the case study model employed here appears to be more suitable for analyzing periods of political stability than times of crisis.

This thoroughly researched study is also marred by serious omissions. For example, the German influence on the development of industrial relations in Schaffhausen, an influence suggested by the large number of skilled German workers and factory managers recruited by the Fischer concern, is scarcely mentioned. Internal adjustments necessitated by rationalization in the 1920s receive only superficial treatment. Omissions like these, combined with the fact that the Georg Fischer firm was not representative of the Swiss steel industry, make it difficult to draw more than tentative conclusions from this book about worker attitudes, organized labor, or industrial relations in Switzerland.

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DALE KENT. *The Rise of the Medici: Faction in Florence, 1426-1434*. New York: Oxford University Press. 1978. Pp. viii, 389. \$37.50.

This book is really the median piece in a trilogy whose other two members are Gene Brucker's *The Civic World of Early Renaissance Florence* (1977) and Nicolai Rubinstein's *The Government of Florence under the Medici (1434 to 1494)* (1966). In this context Dale Kent's monograph is already a significant contribution, but it is made even more important by the exemplary manner in which she performs her task.

Her analysis of patronage networks around the Medici and their political opponents and the transformation of these networks into partisan political groups is a necessary corrective to tendencies to view Florentine politics in terms either of a class-based conflict of ideologies or of a clash of personalities. From the perspective Kent takes, the ultimate political success of the Medici in 1434 appears not simply as the result of greater astuteness on the part of Cosimo de' Medici or of the identification of the Medici with the cause of the poorer elements of Florentine society. Using a wide variety of sources, especially surviving correspondence of the partisans, Kent shows that Medici wealth and financial connections became vital factors in securing favors for friends, many of whom were political newcomers who lacked an established and durable network of social ties and were consequently very dependent upon and loyal to their benefactors. They were the nucleus of Medici political support. In contrast, the anti-Medicean faction contained a number of distinguished Florentines who had long-established social ties, which could generate conflicts of loyalty, and were victims of a generally declining financial position.

One important conclusion Kent comes to about the nature of Florentine politics is that neither extreme (familial or individual activity) should be overemphasized at the expense of the other (pp. 193-94). In this regard one would like a broader discussion of cultural conceptions of political activity than one in fact finds. If there is a shortcoming in this book, it lies in the author's narrow conception of what is political. Kent equates partisanship with the expression of what she terms rational self-interest and assumes that concern for ideological principles dictated neutrality (though one often finds them in the mouths of active partisans). There is no room here for a conception of rationality as culturally determined or of self-interest as informed and interpreted in the light of ideological principles. Political events of such magnitude were not unrelated to fundamental cultural conceptions of the city, family, and honor. One would like further discussion of how Florentines perceived their self-interest materially and symbolically.

Some readers may find the wealth of detail and exhaustive descriptions of partisans difficult going.

The book might have been shorter with no loss to its argument. But its argument is important, cogent, and compelling. There is no doubt that Kent's insights into the operations of patronage networks in Florentine politics and in the establishment of Medici hegemony constitute a splendid contribution that will stand the test of time.

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LORENZO DE' MEDICI. *Lettere*. Volume 1, 1460-1474. Edited by RICCARDO FUBINI. General editor NICOLAI RUBINSTEIN. (Istituto Nazionale di Studi sul Rinascimento.) Florence: Giunti-Barbèra. 1977. Pp. xlv, 584.

The first volume of Lorenzo de' Medici's letters has now been published. It includes introductions by Nicolai Rubinstein, general editor of this international scholarly enterprise, and by Riccardo Fubini, editor of the first two volumes; 169 surviving letters written or dictated by Lorenzo; twenty-one photographs of letters; an appendix, two excursuses, and an index of names. The editors describe the problems of collecting and editing these documents and the rationale for the edition's format. The historical context of each letter is elaborately developed in a preface, which includes explanatory references to other unpublished documents and printed sources, and in notes, which identify the individuals mentioned in the documents. This may well be the most exhaustive, and the most meticulous, edition of a Renaissance *epistolario* that has ever been published. It provides fuller documentation for Lorenzo's career than is available in the most recent scholarly biography by Rochon (1963).

Lorenzo's letters in this volume can be divided into a series of categories: the early familial epistles to *genitori* (but none to other kinfolk); five "humanistic" letters to Marsilio Ficino; four letters to business associates in Bruges; a large batch of letters of recommendation; and an even larger collection of letters pertaining to foreign policy, most of which were sent to Galeazzo Maria Sforza, the lord of Milan. Lorenzo's letters of recommendation include peremptory demands to municipal authorities in the Florentine dominion that they appoint his clients to offices and requests for favors to Italian notables, usually on behalf of Florentine citizens, or someone who had a claim to his support. These requests might be petty, like the note (no. 146) on behalf of "uno Giovannone Ivano da Sarzana, amicissimo mio," or momentous, like the letters in 1472 seeking a cardinal's hat for Lorenzo's brother Giuliano. Lorenzo's correspondence with Galeazzo Maria and other princes reveals the cautious pos-



ture of a young and inexperienced politician, who was learning how to survive in that dangerous world. Writing to Galeazzo Maria, Lorenzo was customarily deferential if not obsequious; he knew how dependent he and his family were on Sforza power. But to other, less powerful correspondents, he could be brutally candid (no. 69) and very shrewd (no. 90). The complex personality of this remarkable Florentine is already apparent in these letters, most of which were written when he was in his early twenties.

This volume is a rich source for the history of fifteenth-century Italy. It will be particularly helpful, indeed indispensable, to future scholars working in Italy's political and diplomatic history. But every student of the late Quattrocento will find valuable information and references in this and later volumes. The editors, the sponsors, and the publishers are to be congratulated on the high quality of this scholarly achievement.

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EGMONT LEE. *Sixtus IV and Men of Letters*. (Temi e testi, number 26.) Rome: Edizioni di Storia e Letteratura. 1978. Pp. 288. L. 12,000.

Francesco della Rovere (1471–84) brought both intellectual qualifications and administrative experience to the papacy. Son of a middle-class family from the tiny northern city of Savona, he spent most of his career teaching philosophy and theology. His election in 1464 as general of the Franciscans brought him prominence, and the scholarly Bessarion, whom he served as confessor and protégé, helped him to obtain the cardinalate in 1467. As pope, however, his scholarly qualities have been overshadowed; he is best known for his large-scale nepotism, by which he sought effectively to govern the Church and Papal States.

He is also remembered as a friend to the Renaissance in Rome. Egmont Lee examines his achievements, evaluating "how a complex historical situation can influence the spirit in which intellectual work is carried out" (p. 9). He analyzes Sixtus's relationship with Roman intellectual, artistic, and architectural achievements, integrating extensive published scholarship with new archival material, especially from the Vatican archives. He concludes that Sixtus combined essentially political motives—the aggrandizement of the Church in the spiritual and temporal spheres—with indifference to the issues that dominated humanist thought.

In documenting the weakness of Sixtus's commitment to Renaissance values, Lee assesses the pope's personal intellectual interests. Although the

evidence is incomplete, it appears that during his pontificate Sixtus neither owned nor read works written by contemporaries. Despite his scholarly background and patronage of humanists, he had little personal interest in Renaissance literary pursuits and in those who followed them. The pope's real criterion for curial appointments was "bureaucratic competence" (p. 202) rather than scholarly excellence. Famous scholars like Argyropoulos, Filelfo, and Platina held curial appointments, but the day-to-day work of church government fell to lesser-known men who combined humanistic learning with administrative skills. Similarly, Lee argues that Sixtus's continuation of Nicholas V's program of urban renewal drew its inspiration from ecclesiastical and practical motives. His aim was the glorification of the papacy, with Rome as its seat. Even his support of the Vatican library, Lee contends, denoted not so much a commitment to the dissemination of learning as the desire to create "a showpiece . . . the pope's gift to the world of letters" (p. 120). Lee's study thus argues the almost accidental quality of Sixtus's patronage of Renaissance architecture and learning in Rome.

Like earlier volumes in this distinguished series, Lee's work is well researched and its arguments carefully presented. A fifty-five-page appendix makes the most significant new archival material available to scholars. The study not only illuminates a vital aspect of the pontificate of Sixtus IV, but it also addresses itself, at least partially, to the growing literature on the role of the patron in the Renaissance. Scholars will use the work to develop comparisons that Lee can only mention briefly: Sixtus's lack of commitment to contemporary learning contrasted with the motives that inspired Nicholas V and Pius II, and Sixtus's Vatican compared with Lorenzo's Florence. Those who concur with Lee's evaluation of Sixtus's patronage will also be moved to speculate on what late Quattrocento Renaissance Rome might have been under a pope whose interest in humanism went beyond an appreciation of its prestige value.

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FRANCIS WILLIAM KENT. *Household and Lineage in Renaissance Florence: The Family Life of the Capponi, Ginori, and Rucellai*. Princeton: Princeton University Press. 1977. Pp. xiii, 325. \$16.50.

Using tax records, letters, diaries, genealogies, and other sources, Francis William Kent has studied three Florentine patrician "lineages" (*consorterie*) in order to contribute not only to our knowledge of Florentine history but also to what we know about



the history of European clan-like families in the Middle Ages.

He begins by looking not at the lineages themselves but at the many individual families, or households, that were their components. Like David Herlihy, and unlike Richard Goldthwaite, Kent finds that the structure of the patrician Florentine household was often large and complex but he does not conclude that the extended family was becoming either more or less common. Rather he shows lucidly and convincingly that households were "living things whose size and nature changed as generations passed" (p. 39). The diary of Giovanni di Paolo Rucellai shows that in his early years he lived in a nuclear family run by his young widowed mother, that later he belonged to a fraternal household with his three brothers, that by 1446 he was the head of his own nuclear family, and that later, after his elder son married and had a child, he was the head of a "grand-family."

But what about the lineage, the larger unit of which the individual households were members? Did the various Rucellai, Ginori, or Capponi households cohere? Did they collectively make up vigorous lineages, each with a strong sense of purpose? Historians of Florence have often maintained, or simply assumed, that by the Renaissance the great Florentine lineages had lost their cohesiveness. Kent shows that at least his three lineages often continued to own property in common, to live near each other in clusters of houses, to take great pride in the lineage name and honor, and to help each other in time of need.

Kent disagrees mildly with Marvin Becker and others who have suggested that the rise of the Medici coincided with a decline in the political activity of the major Florentine patrician families. He shows that the lineages often worked as units to get their members elected to offices, and he demonstrates that, while each of his three lineages supported the Medici, they did so with important differences of style. Kent's chapter on the lineages in politics is perhaps the least satisfactory in his book, but given the complexities of fifteenth-century Florentine politics it is hard to say what else he could have done with the problem.

His study of the three lineages also led Kent to examine their patronage of architecture and painting. It has been suggested that the (supposed) decline both of the extended family and of the lineage was reflected during the Renaissance in a new kind of Florentine family palace that was more "inward looking." Kent disagrees, both because he doubts that there were such changes in households and lineages and because what he has learned about his three lineages' building and remodeling fails to bear out the contention. His findings also lead him

to reject some art historians' suggestions about the "new individualism" of fifteenth-century Florentine painting. Illustrations would have made some of Kent's points clearer, but what he has to say is of great interest.

The result of scholarship both meticulous and imaginative, this study is far richer in findings and suggestions than there has been space to suggest here.

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ANNA MARIA ISASTIA. *Roma nel 1859*. (Istituto per la Storia del Risorgimento Italiano, Biblioteca Scientifica. Series Two, number 32.) Rome: Istituto per la Storia del Risorgimento Italiano. 1978. Pp. 325.

In 1971 the Istituto per la Storia del Risorgimento Italiano published Fiorella Bartoccini's *La "Roma dei Romani"*, an important study of the evolution of the liberal movement at Rome from 1849 until 1870. The Istituto, having spread before us the complex sweep of events considered by this work, has now followed it with another impressively documented monograph that seeks to reconstruct the Rome of the Risorgimento, a monograph whose sharp focus upon a single year among the twenty examined by Bartoccini exposes previously obscure or unknown aspects of the Roman liberal movement. Since, as both Isastia and Bartoccini point out, 1859 was not only a key year but an atypically active one for the Romans, *Roma nel 1859* leaves the reader with the impression of less apathy, of less passivity and contentment than does *La "Roma dei Romani"*. While far from being revolutionary, the Romans, guided by the prudent *Comitato nazionale romano*, made the only type of contribution to the national movement that Cavour wanted at that moment. They contributed men and horses, but above all, by their demonstrations and writings, they conducted "a plebiscite of sympathy" that influenced the foreign press.

The book's strongest emphasis and most fascinating revelations center upon the Romans who volunteered to serve in the armies to the north and upon the role and attitude of the French troops in Rome. Isastia takes us into the cafes where the volunteers were recruited, follows them by railroad and foot to Civitavecchia or into Tuscany, reveals the hardships of their service, the ill-will with which they were often greeted by those whom they sought to help, and the disappointment of the majority who ended up in the Army of Central Italy facing not the Austrians but the pope, whom they had no desire to fight. She shows that the approximately two thousand who volunteered were not "scum," as the conservatives charged, but came from all ranks of

society. While the author describes class participation in this and other aspects of the liberal movement, she makes no attempt to analyze social or economic structures or to provide demographic information.

Others have noted that the French presence at Rome inhibited papal efforts to suppress demonstrations and to block the volunteer effort. Isastia reveals how the French, once war came, have positive aid to the liberals while harassing the conservatives. Until events in Romagna pressed the French back to their traditional (and unpopular) role, they were possibly more of an embarrassment to the papal government than a help. Goyon, Gramont, and other prominent figures receive shrewd attention, and the entire account is placed in its proper international setting.

Though the organization of the study is basically chronological, there is some overlapping of material in sections dealing with the volunteers. Overall, the reader is treated to a clear, sympathetic examination of liberal activity that not only does much to explain how the Romans behaved in 1859 but also suggests why they failed to move in the 1860s.

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CHRISTOPH WEBER. *Kardinäle und Prälaten in den letzten Jahrzehnten des Kirchenstaates: Elite-Rekrutierung, Karriere-Muster und soziale Zusammensetzung der kurialen Führungsschicht zur Zeit Pius' IX. (1846-1878)*. In two volumes. (Päpste und Papsttum, number 13.) Stuttgart: Anton Hiersemann. 1978. Pp. xxxi, 420; vii, 423-833. DM 396.

The cardinals whom Christoph Weber scrutinizes were the last of their kind, controlling the apparatus of state in the central Italian domains of the Church until the 1860s. Through memberships on boards and commissions and occupation of the top-level positions in the state, the thirty-five or forty cardinals formed the upper echelon of the group through which they had risen, the "Roman prelates," who themselves numbered only about two hundred.

The prelates, co-opted from among clerics who could offer proof of sufficient means to support the prelatical dignity, monopolized the second- and third-level positions in the government of the Papal State, maneuvered for positions from which they might rise to the cardinalate, and skirted "the pits" (*i pozzi*, "turkey farms").

What was the relationship between this clerical governing class and the society it ruled? This is the question the author explores. He knows very well that this was the same group that in another capac-

ity staffed the world headquarters of the Roman Catholic Church, but it is the civil bureaucracy and the public that it governed which is the subject of his research in this case.

The peculiar relationship of this officialdom to the rest of the state's population was the object of much critical attention by liberals in the final decades of papal rule, but historians have not given it sufficient attention since. This is the first full-scale attempt to penetrate the issues involved: the influence of family connection on unmarried office-holders, the patron-client element in career patterns, and the workings of necessarily secretive partisanship. Attention has been directed mostly to papal foreign relations, which were all that was left of the pope's sovereignty after 1870. Risorgimento scholarship in particular has neglected the internal constitutions of the Papal State, which might provide the key to some of its riddles (for example, why Romans were such half-hearted Italian patriots).

Historical events as such figure only marginally in this study. What the author does is to thematize the texture of power relationships in the Papal State's ruling class during one long, crisis-racked pontificate so that one can better understand the context of events and their impact. He acknowledges his models for the different disciplinary approaches that have enriched the work (for example, C. Wright Mills and Lewis C. Namier). Using the perceptions of contemporaries as his data, he is able to effect a transformation of recent social science analyses of power structures so that they can be applied to an *ancien régime*, a society underdeveloped economically and politically but with an imposing governmental structure running in well-worn grooves.

It should be noted that the second volume (pp. 423-833) contains only appendixes: chronological career résumés of the cardinals who played a role in the administration of the state under Pius IX; previously unpublished reports on the college of cardinals for various foreign offices; and genealogical tables to help situate such pontifical notables as were connected with Italian and other noble families.

The book will be a valuable resource for church historians but even more for students of nineteenth-century government and society in Europe. It is, as the author claims, a distinct step beyond the history of institutions as conventionally practiced.

PAUL MISNER  
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EMANUEL TURCZYNSKI. *Konfession und Nation: Zur Frühgeschichte der serbischen und rumänischen Nationalbildung*. (Geschichte und Gesellschaft. Bochumer Histo-

rische Studien, number 11.) Düsseldorf: Pädagogischer Verlag Schwann. 1976. Pp. xi, 323. DM 46.

Emanuel Turczynski's book traces the idea of national sovereignty and its influence on the national development of the Serbs and Rumanians within the Austrian Empire back to its beginnings in the medieval churches of these nations. He carefully outlines the social development and Church-sponsored cultural enterprises in each of the areas with predominantly Serbian or Rumanian populations. The result is a creditably complete picture of the national awareness of the Serbs and Rumanians as a function of the rights and privileges that they and their churches were granted, but did not always enjoy, within the Austrian Empire as well as of their social, political, and cultural development.

The book does a good job of placing the Serbs and Rumanians outside the feudal structure, both legally and socially. Its only real shortcoming is a failure to integrate the role of international, great power politics into the development of a national awareness among the Serbs and Rumanians. For example, the wars between Austria and the Ottoman Empire in the late seventeenth and early eighteenth centuries led directly to the Serbian settlements on Austrian lands with grants of privilege that were key to the development of Serbian national awareness.

Turczynski makes an admirable effort to include in his study a good deal of quantifiable data, as is essential in such a broad, social overview. His efforts, however, are sometimes misdirected—clutching at figures for their own sake or drawing facile conclusions for the sake of his thesis. He speaks, for example, of marked increases in the Serbian portion of the urban population in the Bačka in the early eighteenth century, citing the house conscription figures for 1720. But he provides no earlier figures with which to compare these. On the other hand, he concludes from the fact that the priest-household ratio in Srem went from 1:35 in 1766 to 1:78 in 1797 that the position of the priesthood had declined. More likely, the change in ratio reflected a shift from a quasi-farmer peasant priesthood to a professional priesthood—each priest being supported by a larger number of families rather than by the sweat of his own brow.

Such shortcomings are, however, rare and relatively minor. On the whole, the book is an important contribution to our understanding of the development of a national consciousness and the early stages of the national movements among the Serbs and Rumanians within the Austrian Empire.

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LAWRENCE D. ORTON. *The Prague Slav Congress of 1848*. (East European Monographs, number 46.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1978. Pp. vii, 187. \$13.50.

The Prague Slav Congress of literati and national spokesmen in 1848 has a curious place in history. German and Magyar journalists in Vienna and Budapest labeled it a servant of tsarist reaction even before it convened. Nicholas I, who feared it might undermine his multinational empire and upset the European balance, discouraged Russians from attending. The outbreak of armed clashes between rebel students and workers and the Austrian garrison in Prague aborted the congress in early June. Before it disbanded, it approved a "Manifesto to the Nations of Europe"; but its tentative, federalist, Austro-Slav program was never presented to the Habsburg emperor. After the congress numerous legends and controversies arose about its significance, which historians have been sorting out ever since.

Scholarly study of the Prague Slav Congress was long hampered by the disappearance of many of its protocols and minutes. The Czech documents remained unused until 1901 and the Polish until 1927, and reports by Habsburg officials who investigated alleged links between congress participants and unproven revolutionary conspiracies were undisclosed until 1920. Slav and German historians have written much on the subject, and articles on it still appear in East European journals. Lawrence D. Orton expertly surveys and synthesizes this vast output. He winnows fact from supposition, illuminates obscure aspects, and offers a balanced account of the proceedings. As befitting such a polyglot subject, he uses sources in ten languages, identified in numerous reference notes and a comprehensive bibliography.

In a section entitled "The Congress as History," the author details the various interpretations and scholarly contributions, giving due credit to his predecessors but demonstrating his own mastery of the materials. By concentrating on the congress itself, its origins, course, and significance, he proves it to have been an event in its own right, related to the liberal upheaval of 1848 but standing apart from it. The congress crowned decades of a growing national consciousness among the Slavs that hitherto had been expressed mainly in cultural and linguistic forms, yet it was a consciousness of Slavic differences as well as kinships. In seeing the congress from this perspective, Orton carries forward the scholarly mission of his mentor, Václav Záček, the Nestor among contemporary Czech historians of in-

ter-Slav relations. If the author could have gone a bit further, he might have expanded his discussion of those who attended the congress (pp. 63–64) into a full socioeconomic and cultural profile of the Slav intelligentsia.

By coming together on a month's notice and conferring rationally on mutual problems, the Slavs of Eastern Europe stamped the fact of their national existences indelibly upon the European mind. The Prague Slav Congress of 1848 was the first of a half dozen such meetings held prior to World War I, each equally inconclusive. What lingers after one finishes reading Orton's exemplary monograph is the realization that the attitudes and relationships among the Slavs in 1848 prefigured to a startling degree the same ones that have persisted in the twentieth century.

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LESLIE CHARLES TIHANY. *The Baranya Dispute, 1918–1921: Diplomacy in the Vortex of Ideologies*. (East European Monographs, number 35.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1978. Pp. xii, 138. \$11.00.

Within the large sweep of post-World War I political developments in East Central Europe, Leslie Charles Tihany has chosen to study an intriguing illustration of the interplay between domestic politics and external pressures.

The southern Hungarian county of Baranya, with Pécs as its county seat, was occupied by Yugoslav troops following the cessation of hostilities in November 1918. Exploiting the ambiguities inherent in the armistice arrangements, the Yugoslavs intended to stay, hoping eventually to annex the county in spite of its overwhelmingly Magyar and German ethnic composition. To the government in Belgrade, the anticipated permanent possession of rich coal mines near Pécs outweighed even a token adherence to the principle of national self-determination.

In addition, the Yugoslavs, casting ideology aside, supported any group of Hungarians in Baranya who opposed the government in Budapest. The occupation authorities were prepared to view a variety of Hungarian groups as convenient allies in the planned process of Baranya's secession from Hungary, leading to the ironic twists of what Tihany wittily denotes as the "Baranya political cotillion" (p. 33). When the left was in power in Budapest, the Yugoslavs supported the Hungarian rightists in Baranya; when the right seized power in Hungary, Yugoslav support rallied behind the local

Hungarian left. The latter action represented Yugoslav intentions at their most expedient, because support of the Hungarian left in Baranya coincided with suppression of the left within Yugoslavia itself.

Tihany skillfully leads the reader through this succession of shifts, revealing the gap that separated professed beliefs from the imperatives of power politics. He is at his best in describing Yugoslav resolution behind all of the twists and turns of Hungarian political developments. There is a certain confusion, however, when the author equates nationalism with loyalty to a given government. He depicts the Hungarian left in Baranya as "waxing to chauvinism" while Béla Kun's Soviet Republic held sway over the country (p. 29), but then he turns members of the same left into proletarian internationalists once Red Hungary fell (p. 33). The example of Baranya demonstrates that, in times of extreme ideological polarization, political loyalty can shift away from the national center if that center is governed by the ideological adversary. This, however, does not mean that nationalism, as a fundamental sense of belonging to a nation, is a faucet to be turned off and on at will. To take another example, the opposition of German émigrés to the Third Reich did not signify their lack of German nationalism but rather their commitment to anti-Nazi political beliefs.

The compactness of this book is both an asset and a liability. On one hand, Tihany's focus never wavers from the subject; he therefore admirably accomplishes his objective of analyzing the vortex of conflicting loyalties within a clearly defined framework of time and space. But the very compression of the book excludes much of the human element in the story. On the whole, we learn very little about the cast of characters, nor do we benefit from the eyewitness account of the author himself, notwithstanding his promise in the preface. Expanding the subject along this line would be most welcome in any future edition of this valuable study.

GABOR VERMES

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THOMAS SPIRA. *German-Hungarian Relations and the Swabian Problem: From Károlyi to Gömbös, 1919–1936*. (East European Monographs, number 25.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1977. Pp. xi, 382. \$18.50.

Germans were interwar Hungary's only important national minority. Even though they represented less than 10 percent of the population, the Swabians (as Hungarian Germans were called) caused a lot of discomfort for the government and profoundly af-



fected the country's foreign relations. The Swabian problem, the author explains, aggravated the Magyar people's major illness, chauvinism. All Hungarian regimes of the period, whether of the democrat Count Michael Károlyi, of the Communist Béla Kun, or of the counterrevolutionary Admiral Nicholas Horthy, practiced the same basic policy toward the Hungarian Germans: they promised them a minimum of concessions to secure their loyalty and the friendship of Austria and Germany, meanwhile resolutely prodding the Swabians toward the ultimate national goal of assimilation and ethnic homogeneity. In the words of Thomas Spira, "Each government adopted what appeared to be a conciliatory stance and each practiced its evasions with such skill that the Swabians were constantly suspended between hope and despair" (p. 14). Despair seems to have been greater than hope, for the Hungarians oppressed much more than they conciliated, especially in education. The result was that ambitious and urbanized Swabians were driven to become Magyars, while Swabian peasant children were taught very little in either German or Hungarian. No wonder then, the author argues, that most Swabians quickly lost their initial sympathy for independent Hungary and waited only for the opportunity to join the ranks of Hitler's followers.

This Swabian saga is told with much learning and ample documentation; and there are some interesting passages, for instance, those on the clash of ideologies in which nationalism was not the inevitable victor. But there is also some confusion and a few contradictions. The absolute need to discuss both diplomatic and domestic developments, which occurred simultaneously but on vastly different planes, leads to repetitions and to the frequent resurrection, in consecutive chapters, of fallen or dead political leaders. It also leads to a plethora of names that are rarely identified in the text and not always listed in the index. Even such basic terms as Swabian or Saxon are left unexplained and are used rather cavalierly. There is no conclusion, nor is there an indication why this complex story stops in 1936. There are such annoying errors as, for instance, identifying Charles as king of Hungary in 1907 (p. 10) and Count Károlyi as a Social Democrat before October 1918 (p. 16). Finally, the introduction, more than any other part of the book, reads like an anti-Magyar political pamphlet, rather than a scholarly essay. We are told that the Magyars were culturally inferior; that they suffered from feelings of inferiority as newcomers; that, because of this, they were "determined to alter their image by transforming the original Magyar stock through absorption of Indo-European strains," (p. x); that they wished to Magyarize the conquered peoples; and that their assimilationist efforts were

thwarted by the Mongol raids and the Turkish occupation—all involuntary compliments, because they endow the early Hungarians with the political sophistication of a modern nation. Nor does Austria escape censure. We are informed that the Habsburgs systematically subjected Hungary to Germanization, at least until 1867 when they changed to a policy of "fomenting tension amongst their minorities and thereby weakening them," (p. vii), both gross exaggerations. A little more editing and cutting and a little less political passion would have made this lengthy contribution on ethnic history far more valuable.

ISTVAN DEAK  
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WILLIAM H. MCNEILL. *The Metamorphosis of Greece since World War II*. Chicago: University of Chicago Press. 1978. Pp. viii, 264. \$12.95.

William H. McNeill began observing and analyzing affairs in Greece more than three decades ago. His continuing interest in the development of this nation has resulted in no less than three books on modern Greece, which taken together provide a sustained and unique commentary on the country. The latest reflects both the fund of knowledge about Greece that McNeill has built up these past thirty years and the broad perspective of historical change that has become the trademark of his writing.

The appearance of this work is indeed timely. Events in the country these past ten years have brought changes whose ultimate impact will not be clearly manifested in some instances for several more decades. During the last decade Greece has moved out from under the shadow of persistent low productivity characterized by a low living standard. This economic miracle was attained through the inflow of large amounts of credit from abroad, the vigorous commercial efforts of the people themselves, and the significant contribution of a new diaspora of emigrants to all parts of the world. The resulting demographic and social transformation and the recent political crises, both internal and external, have created a great deal of concern about the country's future among Greek intellectuals. In some ways this period is reminiscent of the era just prior to the First World War, when questions concerning the fate of Hellenism in the modern world stimulated the country's creative writers. As then, many of the nation's intellectuals today have been trained abroad, and their analyses of Greek society tend to reflect a reliance on Western theories of development. Even those who see the need for Greece to find its own way to a better social and political



future do so from the perspective of the Western idea of progress. The value of McNeill's survey is that it breaks free of established theories of development to consider both the unique and the universal in the Greek experience.

For centuries the Greek world spread across the eastern Mediterranean making its contribution to succeeding civilizations through communities located in the various urban centers in the area. The establishment of the nation state shifted the focus of attention to one center and its periphery. The dilemma of how to confine the nation within the geographic limits of a territorial state brought a century of military and political conflict. To the traditional view of the vicissitudes of Hellenism over the last one hundred and fifty years McNeill seeks out and adds new points of reference.

Since the time of Odysseus, Greeks have exuberantly proclaimed their individual place of origin. Historians of Greece have often noted the continuing role of geographic diversity in shaping modern Greek society. But McNeill fixes upon this verity and probes its full meaning to establish the patterns of life that have been fundamental to Greeks down to this day. From the author's survey, which combines history and contemporary observation, there emerges a picture of a people full of contradictions. We see the antipodes of food-deficit and food-producing villages; of the heroic versus the calculating, entrepreneurial spirit; of the secular and the devoutly Orthodox individual; the hill and the plains people; and, finally, of the rural and urban world in Greece. By combining these often conflicting tendencies within their culture the Greeks have produced a vigorous society that is both enduring and unique in McNeill's estimation.

This is a work that has something to offer even to those most knowledgeable about modern Greek life. It is a luminous example of how interpreting the past can serve to make the present more intelligible and the future less of an enigma.

GERASIMOS AUGUSTINOS  
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*Patriot or Traitor: The Case of General Mihailovich. Proceedings and Report of the Commission of Inquiry of the Committee for a Fair Trial for Draja Mihailovich.* Introductory essay by DAVID MARTIN. Foreword by FRANK J. LAUSCHE. (Hoover Institution Publication, number 191.) Stanford: Hoover Institution Press. 1978. Pp. xviii, 499. \$19.00.

The alleged collaboration of the Chetniks with the Axis occupiers, especially against Tito's Partisans, and the culpability of the Chetnik leader, General Draža Mihajlović, have been highly controversial

and emotional issues in Yugoslavia and Yugoslav historiography since the actual events upon which these questions are based took place. It was this apparent treachery that led in part to the shift of most Allied and popular support from Mihajlović to Tito in the closing years of World War II. But even the trial and execution of Mihajlović for treason in 1946 by the Tito government did not settle the matter. Although most current historical writing substantiates the Chetniks' and Mihajlović's guilt, some critics label this scholarship "revisionist" and incomplete, claiming that it is based on faulty research. They argue that, although a great deal of documentation is readily available, much of it is from the Tito government's archives and other pro-Partisan and pro-Communist sources.

This volume in the Hoover Institution's archival documentary series attempts to shed new light on the case of Mihajlović and the Chetniks. The book is composed of the complete Proceedings and Report of the Commission of Inquiry of the Committee for a Fair Trial for Draja Mihailovich, reproduced here for the first time, with a lengthy introductory essay by David Martin, a founder of the Committee for a Fair Trial for Draja Mihailovich and the author of a study on Allied relations with Tito and Mihajlović published in 1946. Most of Martin's remarks deal with the activities of the committee, a phenomenon of the Cold War, whose membership was made up largely of hundreds of Allied airmen who went down over Yugoslavia and who were rescued by the Chetniks. The committee sought unsuccessfully to have the testimonies of these airmen in defense of Mihajlović placed in evidence at his trial. They testified that, in all the time they collectively spent with the Chetniks, there were no indications of collaboration. Also included in this volume is some British archival material supporting the committee's contention that the British-led Allied "abandonment" of Mihajlović and the Chetniks was to a large extent based on poor and slanted intelligence.

A major implication of this book is that Mihajlović's crime was that he resisted the Communists as vigorously as the fascists; if there was any collaboration on the part of the Chetniks, it was by "peripheral units" under local commanders. If this is a valid view, it is not substantiated by the evidence offered here. Neither does this construction really address the broader, more complex situation of Allied cooperation between Chetniks and Partisans, capitalists and Communists against the real and immediate threat of fascism during World War II. The documentation supplied by this extremely interesting volume nevertheless will be valuable to scholars, students, and informed readers alike in achieving a better understanding of the complexity

of the Yugoslav Revolution, especially when read in conjunction with Walter R. Robert's *Tito, Mihailovic and the Allies, 1941-1945* (1973), Jozo Tomasevich's *The Chetniks* (1975), Milovan Djilas's *Wartime* (1977), and others.

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VLADIMIR V. KUSIN. *From Dubček to Charter 77: A Study of "Normalization" in Czechoslovakia, 1968-1978*. New York: St. Martin's Press. 1978. Pp. x, 353. \$18.95.

Although dozens of books explaining the events of 1968 in Czechoslovakia line library shelves, practically nothing has appeared describing what has transpired since the Soviet Union and its allies offered their "fraternal assistance" and Gustáv Husák ousted Alexander Dubček from the Communist Party helm in April 1969. For that reason alone, Vladimir V. Kusin's fact-filled book, his third on contemporary Czechoslovakia, is an important contribution.

Relying on a mixture of material painstakingly gathered from the Czech press, as well as from émigré journals and an occasional unnamed source, Kusin seeks to explain the political events of the decade since 1968. He divides the period into four parts: the eight months of Dubček's leadership following the invasion, consolidation and purges (1969-71), attempts to win over the masses (1971-76), and the renewed opposition movement he sees in Charter 77.

Although Kusin calls his book "an exercise in analysis and argumentation," he fails to come to grips with, or even to raise, the complicating questions, many of them historical, that condition contemporary developments. He argues, for example, that the coexistence of democracy with socialism for Czechoslovakia has been a matter of historical experience. Is this not, however, descriptive of form, rather than content? The modern Czechoslovak tradition is one more of organization than of democracy, of government for the people, not by the people. The Masaryk tradition is less one of democracy than of enlightened stewardship by intellectuals.

Nor, as Kusin suggests, did Husák in the late 1960s try to impress Soviet leaders with his abilities to organize and to get a job done. An examination of Husák's career, in his student days in Bratislava in the 1930s or as a Slovak leader in the 1940s, argues persuasively against that view; he has never believed it necessary to prove himself to anyone. Husák has always been a tireless toiler who disdains excessive debate and discussion.

This is a distinctly Prague's eye view of events.

An observer writing, for instance, from Ostrava-Haviřov, the big industrial center in northern Moravia, would describe a much more satisfied populace than the one Kusin finds in Prague. Kusin makes only a few allusions to the generally more contented citizens of Slovakia. In many ways Husák has been a success, and Kusin seems unwilling to explain in detail why. For example, the economy showed steady progress from 1966 to 1978. Is not this accomplishment at least as important as Husák's political problems? This might help explain why the populace has given Charter 77 little more than sympathetic understanding, although Kusin waxes emotional about it, describing it and quoting from it at great length.

There is also nothing in this book about the alliances between various party leaders in Prague and the regional leaderships. Yet, as much excellent scholarship by Western students of the Soviet Union on this subject has shown, there is much to be learned in such studies. Kusin chastises Westerners for failing to detect the movement in Czechoslovak ruling circles in the early 1960s that anticipated the Prague Spring, but he essentially denies that the regime today might be anything but monolithic.

Kusin's book turns out to be an important first step, a narrow accounting of politics at the center. It invites further research in order that we may better understand this important chapter in contemporary European history.

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ALEXANDRU DUȚU. *Cultura română în civilizația europeană modernă* [Rumanian Culture within Modern European Civilization]. Bucharest: Editura Minerva. 1978. Pp. 266. 7.75 L.

That Alexandru Duțu is perhaps Rumania's most distinguished student of the history of ideas in eighteenth- and nineteenth-century Southeast Europe is demonstrated by this important new book. Rejecting on the one hand approaches that "establish the coefficient of enlightenment [in Southeast Europe] as a function of the percentage of contacts with the West" and on the other hand condemning "the naive conviction that the indigenous product is always superior to the foreign" (p. 106), this extended and complex essay traces the evolution of modern European culture during and after the Enlightenment and explores its relationship to Southeast Europe (particularly Rumania). The first two chapters review the intellectual history of both Eastern and Western Europe since the seventeenth century. The picture of the Enlightenment and its effects is well

drawn and well informed by current literature—Eastern and Western—on the subject. It shows that the traditional stereotype of a Southeast Europe vegetating, contrasted with a West transforming itself dynamically on every front, is based on a failure to understand the long-run cultural, political, and social dynamics of the region. These “coordinates of culture” were, in turn, not simply the product of an extended isolation from the West, but were produced by differing heritages and circumstances as well as by the interaction of East and West. The argument is based on a number of suggestive comparisons and contrasts—for example, the continuity of antiquity in Byzantine and Southeast European tradition as opposed to the transformation of the classical heritage in the medieval synthesis of the West (pp. 29ff.). Remarkably, in contrast to most works of this sort, these suggestions are almost never forced to carry more weight than the reasonable reader can be expected to accept. Remarkable, too, is Duțu’s firm recognition that merely identifying “influences” without unraveling the ways in which “borrowed” ideas function in the literary work of an era and region reduces “cultural history to textology and the concerns of men to amusing caprices” (p. 60).

It is Duțu’s efforts in avoiding these pitfalls that make this book so stimulating and significant. One key result of this analysis is to show fairly convincingly how the process of the Enlightenment in Southeast Europe produced the philosopher-patriot, the nationalist scholar whose interest in the vicissitudes of his people often overrode the requirements of objectivity. The same analysis, in chapter two, also shows why the “heroic” approach to history (whether the Haiduc myths or national standard bearers) became standard fare. This evolution was of critical importance in political developments of the nineteenth century.

Chapter three elaborates on the scholarly result. Ironically, the production of politically useful “selective traditions” and “golden age” mythologies led to the Western misperception of Southeast European history alluded to earlier. Duțu places the blame for this squarely on the romantic and/or positivist schemata of Southeast European literary and cultural historians, while calling for and beginning a reappraisal of many commonly held conclusions.

The second half of the book consists of the application of previously developed approaches and ideas to Rumanian history. The focus is on long-run influences (in the Braudelien sense) that were important in producing the nineteenth-century Rumanian cultural and political synthesis. Again, in remarkable contrast to similar works, these influences are actually demonstrated rather than alleged

or proof-texted. No one interested in the development of modern Rumania will fail to profit from the analysis.

This is a most impressive and penetrating work. Though its primary audience is those interested in Rumania, it could alter several generally held misconceptions about Southeast European history if its ideas were more widely accessible. It is to be hoped that a Western-language edition will soon appear.

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KAZIMIERA JANINA COTTAM. *Boleslaw Limanowski (1835–1935): A Study in Socialism and Nationalism*. (East European Monographs, number 41.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1978. Pp. xvi, 365. \$18.50.

The core of Kazimiera Janina Cottam’s book—a revised version of her dissertation, which won the Kosciuszko Foundation Doctoral Award for 1972—is an intellectual biography of Boleslaw Limanowski. Limanowski, born in Latvia in 1835, educated at Moscow secondary schools, the University of Moscow, and the German university at Dorpat, was old enough to participate in the Polish revolutionary movement of the late 1850s and early 1860s and still young enough in 1929, at ninety-four, to address a “vehement letter to President Moscicki, openly branding the [Pilsudski] government as a corrupt ‘dictatorship’” (p. 215). In the seven decades between these two events, Limanowski’s career mirrored much of the political, social, and intellectual history of modern Poland.

Limanowski was a versatile writer, a distinguished historian-scholar, a political activist as well as theoretician, a sociologist, and one of Europe’s earliest feminists. His intellectual mentors were, among others, Condorcet, Saint-Simon, Lasalle, Comte, Buckle, Mill, Darwin, Bagehot, and Spencer. Limanowski, Cottam says, “absorbed all intellectual trends to which he was exposed” (p. viii).

The introductory chapter of Cottam’s book provides the necessary political, social, and ideological background to an understanding of Poland’s agitated history, and, more particularly, acquaints the reader with Limanowski’s Polish intellectual and revolutionary heritage. The seven subsequent chapters tell the story of his unique intellectual, as well as geographical, odyssey: as a publicist and socialist agitator in Galicia; as an exile for the socialist cause in Geneva; as a spokesman for the Polish League, a new émigré organization dedicated to a renewal of the struggle for Polish independence, in Thun and Zurich; and as a proponent of the idea “that Polish

socialists in partitioned Poland should first of all aim at national liberation, both as an end in itself and as a means of achieving socialism" (p. 241). The final four chapters cover: (1) Limanowski's activities in Cracow in the decade before 1918, when, in alliance with Pilsudski, he facilitated the realization of his own, and Pilsudski's, patriotic aim—Polish political independence; (2) the trials and tribulations of independent Poland and Limanowski's views on the major political, social, and economic problems, including Pilsudski's dictatorship, troubling the nation; (3) an evaluation of Limanowski's contributions to political philosophy; and (4) an estimate of his place in history.

In the preface to her book, Cottam notes that, although numerous sketches of Limanowski have been published, "there is as yet no comprehensive political biography written in the West, synthesizing all his views and attempting a critical analysis of . . . [his] activity from a historical perspective" (p. x). This deficiency she decided to correct; she has succeeded with professional aplomb. Altogether, the book is a fine piece of research and analysis. Sources, published and unpublished, include government documents, memoirs, diaries, private correspondence, and periodical literature. The scholarly apparatus, with voluminous footnotes and rich bibliography, is impeccable.

The book, however, is flawed by a rather substantial number of misspelled words, missing lines, and grammatical errors; and the omission of diacritical marks in Polish spelling detracts from its scholarly tone. These observations are not meant to diminish the value of Cottam's solid, perceptive, and sometimes brilliant study.

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PETER RAINA. *Political Opposition in Poland, 1954–1977*. London: Poets' and Painters' Press. 1978. Pp. 584. \$12.00.

This is a highly useful book that ought to be of considerable interest not only to students of recent Polish political history but also to those specializing in the fields of international communism and authoritarian politics. There is no doubt that the author, a native of India who received his doctorate in Warsaw, is very much at home with the subject of political opposition in Poland, especially during the 1960s. Although, for obvious reasons, he is reticent about it, it is clear that he has been personally well acquainted with many of the *dramatis personae* mentioned in his book who played a major role in leading the opposition groupings and who provided him with some first-hand accounts and insights into the

frequently labyrinthine and occasionally bizarre aspects of Polish politics in the past twenty-five years. It is, most likely, because of his intimate knowledge of the various conflicts and splits that characterized Polish Communist rule that he was eventually proclaimed *persona non grata* and forced to leave the country.

Although most of the facts and events reported by Raina have been well known in the West for some time, these personal insights and hitherto unknown details make the volume particularly valuable, if only by tying some loose ends and filling a few remaining gaps in our knowledge of recent developments in Poland. A more important contribution made by the volume, however, is that it explodes once again two still widely held myths and beliefs: one, of the "totalitarian" character of contemporary Communist rule in Eastern Europe and, two, of the absence of "politics" in Communist systems.

Of the two, it is perhaps the second myth that is of particular interest. Conventional wisdom has long implied that, since Communist rule meant by definition total control by the center (be it a single leader or an oligarchy), there is no room for politics, which above all suggests a struggle for power among several competing parties or groups, each with a different set of goals and objectives. Now and then, there are references in the literature to a phenomenon of "crypto-politics," but the term has not become widely accepted except by a few specialists in the field. The majority prefer for reasons of their own to adhere to the traditional perception of communism as a totalitarian or neototalitarian creed that allows no competition, not to mention formal opposition or dissent. As suggested earlier, this volume should go a long way toward refuting that view. The author traces the modest beginnings of dissent that emerged in Poland shortly after Stalin's death in 1953 and that has shown a gradual yet steady growth throughout the past quarter of a century until today, when political opposition to the regime has become quite formidable. Space does not permit delving deeper into some of the more fascinating individual case studies of dissent and opposition that well illustrate the thesis that Communist rule in Poland and, to some extent, elsewhere in Eastern Europe has departed quite far from the textbook model that is still so familiar to most Americans.

In fact, after reading Raina's study I would be prepared to argue that the process of divergence from the norm has gone so far that in many ways the Communist rulers of Poland, as well as those of Yugoslavia and possibly of Hungary, frequently find the task of governing their respective countries much more difficult and frustrating than their dem-



ocratic counterparts in the West. Much more research is clearly needed to substantiate this somewhat paradoxical thesis, but in this respect the book represents a good beginning and makes a valuable contribution.

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EDGARS DUNSDORFS. *Kārļa Ulmaņa dzīve: Ceļnieks, Politīķis, Diktators, Mocekļis* [The Life of Karlis Ulmanis: The Voyager, the Politician, the Dictator, the Martyr]. Summary in English. Stockholm: Daugava. 1978. Pp. 613. \$29.50.

Karlis Ulmanis was the most visible and important statesman in the period of independent Latvia (1918–40), and as such he deserves a thorough scrutiny by historians. This is the first critical biography of him by a major Latvian scholar.

Ulmanis had a checkered career. Born in 1877 to a family of Latvian landowning peasantry, he studied agronomy at Leipzig University. In 1906, because of some congratulatory articles he had written a year before concerning the 1905 Revolution, he considered it prudent to leave his home for the United States. In this country, among other things, he continued his study of agronomy at the University of Nebraska. In 1913, having learned that it was safe to return, he departed for Latvia, where through lectures and publications he commenced intensive educational work among his countrymen, sharing with them his knowledge in scientific agriculture. Thus, he began to make a name for himself among Latvia's farmers and social leaders. By the beginning of the 1917 Revolution, he emerged as the most visible spokesman for Latvia's rural population and was one of the leaders in establishing the Peasant Union Party, which in 1917 and thereafter became the most pronounced antagonist of Latvia's Social Democratic Party. In 1918 Ulmanis became the first prime minister of independent Latvia, a function that he exercised on several other occasions during the 1920s. He remained in the leadership of the Peasant Union from the chaotic revolutionary years through the parliamentary period of Latvia, which was ended by his own coup d'état in 1934.

In 1934 Ulmanis became the first and only dictator of Latvia, suspending the existing constitution. Though he was a contemporary of Hitler and Mussolini and his regime had certain characteristics of fascism, his order was also different enough to set it apart from the major European dictatorial prototypes. In 1940, after the Soviet takeover of Latvia, Ulmanis was arrested and exiled to the Soviet Union, where he died in 1942.

Considering that he had no monographic studies on Ulmanis's career to rely on, Edgars Dunsdorfs has done a creditable job in writing the biography. The work is a summary of memoir literature written, for the most part, by exiled politicians since 1940. Recognizing that they are contradictory, Dunsdorfs does not so much draw conclusions from these memoirs as confront one memoir account with another, allowing the reader to choose.

Although Dunsdorfs has not written anything close to a definitive biography, for which he would need archival materials, one can say that he has assembled much material and clarified many issues about Ulmanis's career. All future historians working on Ulmanis will be grateful to Dunsdorfs for the work that he has accomplished.

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E. N. BAKLANOVA. *Krest'ianskii dvor i obshchina na russkom Severe: Konets XVII-nachalo XVIII v.* [The Peasant Household and Commune in the Russian North: The Late Seventeenth Century to the Early Eighteenth Century]. Moscow: Izdatel'stvo "Nauka." 1976. Pp. 221. 98 k.

P. A. KOLESNIKOV. *Severnaia derevnia v XV-pervoi polovine XIX veka: K voprosu ob evoliutsii agrarnykh otnoshenii v russkom gosudarstve* [The Northern Village in the Fifteenth through the First Half of the Nineteenth Centuries: On the Problem of the Evolution of Agrarian Relations in the Russian State]. Vologda: Severo-Zapadnoe knizhnoe izdatel'stvo. 1976. Pp. 415. 2 r. 10 k.

Soviet engineers working in the Russian north have found ways to "float" their buildings, because the soil, when pressed upon too heavily, turns mushy. In their excellent though very different books, P. A. Kolesnikov and E. N. Baklanova demonstrate that Soviet historians have also mastered the art of floating their constructions (or reconstructions) in something of a quagmire. The archival materials they necessarily build upon are skillfully made to bear great numbers of impressive small and medium level generalizations about peasant economic life in the Russian north—without crushing a single detail. And, significantly, the Marxist-Leninist cement that helps to hold things together is relatively unobtrusive in and among the concrete cases that bear the weight. Both studies come out of a provincial center of learning and deal with a peculiarly Russian historical milieu, but they draw upon and add to an already extensive literature on the subject matter and there is much in them that ought to interest even non-Soviet historians and social scientists.



Kolesnikov's work is the more important of the two. The book is somewhat uneven, because the author draws heavily in some sections upon his own previous work. The sixteenth and seventeenth centuries, for example, are treated more intensively than the others; migration toward the Urals and Siberia is a constant reference; the yields of different crops and the "balance" of animals, fertilizer, hands, and other factors in the peasant economy are emphasized. For the nineteenth century we learn mostly about the work and findings of the "Northern School Farm." The broader picture, nevertheless, emerges successfully. With great love and obvious admiration, Kolesnikov traces the settlement of northern European Russia by a relatively free peasantry. He stresses that settlement took place in the easier areas along rivers and on natural meadows and was not hacked out in the virgin forest. The predominant form of settlement whenever possible was the relatively permanent village (*derevnia*) and not the isolated farmstead in a "clearing." The preferred system of cultivation was the three-field rotation rather than cut and burn, although necessity often drove peasants to seek the higher initial yields of the latter. Kolesnikov characterizes as a myth the idea that monks and religious institutions opened the north—they came later, he claims, settled near villages, and contested with the peasantry for control of already productive land. The immense difficulty of establishing and maintaining agriculture in the north is illustrated at length, and so is the long decline that began sometime in the seventeenth century.

The peasant economy in the north was viable in spite of natural difficulties. In the sixteenth century it flourished. It was ruined, Kolesnikov is persuaded, by the gradual encroachment of feudal landlords and—more significantly—by the intolerable burdens imposed by the state. Most notoriously in Petrine times but continuously thereafter, brutally exacted taxes, recruitment of soldiers, and forced labor drained peasant households of men in their best working years and of indispensable animals. Land fell out of cultivation or returned smaller yields, pauperized families "went on the *mir*," those who could do so migrated eastward or abandoned agriculture to seek employment in commerce or in the nascent industry of the area. State intervention, even when well intentioned, only made matters worse. Kolesnikov's description is persuasive; his codicil that only the socialization of agriculture could remedy the situation is probably extraneous.

Baklanova focuses on a smaller area (Vologda *uezd*) and a shorter period (1675–1725). In her dissertation she provides us with a stream of quotations from her primary sources and an avalanche of

detail that would surely bury us were it not for her heroic pointing up of lower level conclusions on every other page. She advisedly leaves larger generalizations to such as Kolesnikov, but she does not write in a vacuum and is able to insist that her information on the peasants of two large *votchina* estates (church land in this case) in the Vologda area, just south of the "black" peasantry of the Russian north, is relevant for that population and vice versa. In spite of their serfdom, Baklanova tells us, her peasants had much greater control (within the *votchina*, of course) of "their" lands and economic affairs than has been assumed. This she adumbrates at length.

Baklanova describes her peasant household (*dvor*) in great detail, pointing out typicalities without blotting out the range of variation. Size and composition of families is examined. In accordance with Lenin's suggestion, the number of workers per household is stressed. But Baklanova rides the requirement easily while presenting her information on land allotments, numbers and types of animals, implements, taxes and other burdens, and the evolution of all this over time. The impact of Peter's demands upon the economy (ruinous) is amply illustrated.

Kolesnikov and Baklanova are both generous with bibliographical information. The former, however, should have provided at least one map, and the latter should have given us a glossary.

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SH. B. CHIMITDORZHIEV. *Vzaimootnosheniia Mongolii i Rossii v XVII-XVIII vv.* [Relations between Mongolia and Russia in the Seventeenth and Eighteenth Centuries]. Moscow: Izdatel'stvo "Nauka." 1978. Pp. 214. 1 r. 30 k.

The relations between Mongols and Russians from the beginning of the seventeenth century to the mid-eighteenth is only one of several themes, and not always the dominant one, of this small but contributive monograph. Russian-Manchu intercourse (both diplomatic and commercial), Manchu-Mongol rivalry, and the internal struggles among the Mongol tribes for supremacy in the region and independence from outside powers impinged upon direct Russian-Mongol contacts and often governed them. The main contribution of this study (based substantially on Soviet and Mongol archives, although inadequate in its use of recent Western scholarship) is the detailing of the diplomatic "embassies" and communications between Mongol princes and Russian and Siberian authorities beginning with the 1606 journey of a representative of

the Oirat (western Mongol) prince to the Russian *voevoda* at Tara, which was followed by an embassy to Tsar Vasiliï in Moscow at the end of the next year to plead for military aid. Not until 1619 did the first Russian ambassador visit Oiratia, but in the mean time (1608) the Russians attempted an approach to the Altan-khan of Khalkha-Mongolia to the east. Over the years thereafter fairly lively trade grew between the Mongols and Russians, and diplomatic embassies, themselves partly commercial, traveled in both directions at irregular intervals. Still, the crucial factors in this intercourse lay elsewhere: the primary Russian goal was China, not Mongolia; the main dynamic was trade, not diplomacy. The Manchus for their part were principally motivated by the extension of their peculiar tributary-trade system to Mongol lands. For the Russians, Mongol affairs were subsidiary to Chinese; for the Manchus, Russian affairs were almost always secondary to Mongol. The Mongol tribes repeatedly were pawns in the games the two greater powers played until Manchu armies overwhelmed the last holdouts early in the second half of the eighteenth century.

Sh. B. Chimitdorzhiev seems inordinately interested in establishing motives: the Manchus invariably were aggressive and deceitful; the Russians peaceful, friendly, and generous; the Mongols pure in their love of liberty and desirous of amicable dealings with Russians. It is here in the matter of motive that he is least persuasive and fails to make a close fit between evidence and conclusion. He can cite little direct primary evidence regarding the intentions of Russians, Manchus, or Mongols, and his inference from observable act is often not credible. He struggles, for instance, to prove that the Transbaikalian Buriats happily accepted Russian suzerainty but other worthy Mongols invariably rejected Manchu dominion, which subverts rather than fulfills.

Not unusual for Soviet publications, this book cries for even the simplest of maps and a subject index.

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B. L. FONKICH. *Grechesko-russkie kul'turnye svyazi v XV-XVII vv. (Grecheskie rukopisi v Rossii)* [Greek-Russian Cultural Ties in the Fifteenth to Seventeenth Centuries (Greek Manuscripts in Russia)]. Moscow: Izdatel'stvo "Nauka." 1977. Pp. 245. 1 r. 50 k.

This work is a most significant effort by a leading Soviet codicologist, Boris L. Fonkich. He has amassed a wealth of information, providing us with a thorough history of the transmittal and retranscription of major Greek (Byzantine) manuscripts

and published books, primarily religious works, in late Muscovite and early imperial Russia. The main emphasis of his study, however, is to trace the "lives" of the texts, and he only concerns himself with those that are extant.

Fonkich has divided his work into four parts. First, he examines Greek retranscription in Russia during the fifteenth and sixteenth centuries. Twenty-three texts are studied, including an anthology of 1408, a trilogy of the same year, a book of gospels dating from the 1480s or 1490s, and a psalter of 1540. Only eight texts date from the fifteenth century and the remainder belong to the next. In part one, Fonkich also establishes the methodology that he faithfully preserves throughout the book. He provides us with a detailed description of the origin (when known) and essential characteristics of each text, comparing corruptions and omissions as well as significant alterations or additions. Through a reconstruction of the history of the texts, Fonkich reveals the names of churchmen who inspired the solicitation of works, of monks who journeyed to the Near East and to Mount Athos and its numerous monasteries to gather religious works, and of the copyists, readers, and even students of the Greek texts that arrived on Russian soil. The main figure in part one is Maxim the Greek, a humanist identified with the Italian Renaissance. Maxim arrived in Russia in 1515, at the invitation of Muscovite Grand Prince Vasiliï III, for the express purpose of correctly translating Greek texts into Russian. Fonkich attributes only one major translation to Maxim, a psalter that appeared in 1540 (pp. 45-49). Fonkich does not stress in this part the importance of Mount Athos, where Maxim had entered religious life, as a new and important center for translation and transcribing activity and one that would shortly play a major role in the Russian literary and cultural revival.

The second part of this book deals exclusively with the manuscript and book gathering of Arsenii Sukhanov, a monk of the Trinity Monastery. In 1649 Arsenii was dispatched to the Near East and to Mount Athos to buy manuscripts and printed editions of Greek church books. He returned four years later with a vast and immensely valuable collection numbering over five hundred items, including gospels, bibles, missals, and other religious texts. These Fonkich identifies only by number at their place of deposit. He does trace the route of the major works to the numerous monasterial depositories in Russia.

Fonkich traces the "lives" of the Arsenii collection in parts three and four, noting with great detail their retranscription and retransmittal to numerous monasterial depositories in the course of the second

half of the seventeenth century. In sum, Fonkich for comparative purposes has studied over three thousand manuscripts and printed works. His important codicological studies demonstrate that Greek-Russian cultural relations of the fifteenth through the seventeenth centuries, though confined to the religious sphere, brought Russia out of a cultural desert.

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ALAN FISHER. *The Crimean Tatars*. (Studies of Nationalities in the USSR.) Stanford, Calif.: Hoover Institution Press. 1978. Pp. xii, 264. \$14.95.

This work presents an extensive survey of the historical experience of the Crimean Tatar people under the khanate, imperial Russian rule, and Soviet power. Alan W. Fisher established his impressive command of the relevant source materials in a series of earlier publications. Here he presents those materials and much more in a careful blend of historical detail and analytical commentary. The result is a text that provides both a comprehensive introduction to the topic for beginning students in Soviet history and politics and a literature review and bibliographic guide for more advanced students interested in pursuing further research on any one of the several suggestive hypotheses or unanswered questions about Tatar and Soviet history presented here. For the specialist in Soviet Muslim history or Soviet nationality politics, on the other hand, this work does not present any new material or analyses. On the contrary, Fisher is careful to point out clearly where the available materials are inadequate to allow a definite interpretation, rather than to break any new ground here.

*The Crimean Tatars* is intended as the first in a series of volumes on the history and development of the non-Russian nationalities of the Soviet Union. If this series is to fill the existing gap in historical and political studies of these peoples, however, future volumes will have to devote greater analytical attention to the Soviet period and especially to the period since World War II. Fisher presents convincing evidence to dispute Soviet historiography concerning the role of the Tatars during World War II and to account for the deportation of the Tatars to Central Asia. But he does not offer any critical discussion of the role or importance of the Tatar national movement since that time. Instead, he restricts himself to a straightforward recitation of events, devoid of significant analytical commentary of his own. Moreover, there is no account at all of events since 1970, a surprising shortcoming for a book published in 1978.

The Crimean Tatars were deported to a largely Muslim area of the Soviet Union. What has been the effect, if any, of the Tatar national movement on the local Muslim intelligentsia? What has been the reaction of the local Muslim population to violent demonstrations by Tatar activists? And what are the implications for Muslim-European relations elsewhere in the USSR of a massive return of the Tatars to the Crimea? Had Fisher turned his formidable analytical skills to these questions, he not only would have provided his readers with important insights into the dynamics of national dissent in the Soviet Union today, but he also might have answered the question that seems to haunt the last third of this volume: Why do Soviet authorities still refuse to allow the Tatars to return to the Crimea?

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N. I. TSIMBAEV. *I. S. Aksakov v obshchestvennoi zhizni poreformnoi Rossii* [I. S. Aksakov in the Public Life of Postreform Russia]. Moscow: Izdatel'stvo Moskovskogo universiteta. 1978. Pp. 262. 2 r.

It is a pleasure to welcome a good and useful book. The pleasure is double when the book in question is a Soviet study of the Slavophiles, more precisely of Ivan Aksakov in postreform Russia. The author, N. I. Tsimbaev, begins with an introduction, a careful discussion of the sources, and a survey of the literature on his subject. The narrative proper contains five chapters: they cover the social and political views of Ivan Aksakov in the 1840s and 1850s, his role as the editor and publisher of *Den'* [Day], 1861-65, his production of the newspaper *Moskva* [Moscow], 1867-68, his sociological scheme in the 1860s, and his social and political activity in the 1870s and 1880s. These chapters are followed by a brief conclusion, a name index, and a table of contents. The book is attractively presented and remarkably free of misprints (I found two in the entire volume). And it costs only two rubles!

Although welcome, the study is not without its faults. Like other Soviet scholars, Tsimbaev is weakest in his fundamental conceptualization of the Slavophiles, whom he interprets, in the wake of S. S. Dmitriev and Dmitriev's disciples, as relatively liberal and objectively bourgeois. That approach allows more for Slavophilism than other Soviet approaches, but it is still a far cry from trying to understand the ideology and the group in their own terms. Also, in spite of being unexceptionably scholarly on the whole, Tsimbaev engages occasionally in gratuitous attacks on Western historians (pp. 28-29) or refers to abstract Marxist schemes not relevant to his narrative. He can even be criticized for a

fault rare among Soviet students of the Slavophiles, but otherwise common among authors, that of overemphasizing the significance and quality of the subject. Few would join Tsimbaev in calling Ivan Aksakov "a major and original thinker" (p. 4) or in assigning such great importance to his concept of "society," which, as the study itself so well demonstrates, was based on the thinking of older Slavophiles and generally held by Ivan Aksakov in an erratic and tentative manner until he finally abandoned it.

Yet, the author does his work very well. With a skillful, at times masterful, utilization of printed and archival sources, he presents a rich, full, nuanced, convincing, even compelling account of Ivan Aksakov's important publicist and other activities in the course of several decades. He grants his hero total sincerity but notes his repeated inability to gauge reality correctly and his full share of responsibility in the constant conflicts with ideological opponents, coworkers, and especially the government. Although much by and about Ivan Aksakov has already been published and although Tsimbaev's study provides no major revelations, it does serve to enrich greatly our knowledge of Ivan Aksakov with new material that often makes important points more effectively than they had ever been made before.

We all are in N. I. Tsimbaev's debt.

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HEINZ-DIETRICH LÖWE. *Antisemitismus und reaktionäre Utopie: Russischer Konservatismus im Kampf gegen den Wandel von Staat und Gesellschaft, 1890-1917*. (Historische Perspektiven, number 13.) Hamburg: Hoffmann and Campe. 1978. Pp. 303.

This is the most comprehensive and serious study yet written on anti-Semitism in imperial Russia, and it is excellent. It eschews moralizing and such one-dimensional explanations as cynical scapegoating, national-religious fanaticism, and economic envy or competition. It draws upon a rich body of sources in languages other than Hebrew or Yiddish. Archival materials, even if accessible or extant, are unlikely to compel substantial revisions of the factual background presented by the author.

Proceeding chronologically (from 1890 to 1917) and topically, Heinz-Dietrich Löwe deals judiciously with the "peculiar" position of Jews as outsiders in an agrarian society, their part in the revolutionary movement, the political and ideological dimensions of the Jewish question, and the role of officials in promoting, supporting, or tolerating

anti-Jewish movements and excesses. The question why a conservative regime would risk calling unruly masses into the street can no longer be regarded as a misguided attempt to restore the good name of tsarism.

Löwe concludes that it is neither probable nor provable that the Kishinev pogrom was organized by Plehve and that it is unnecessary as well, since his subordinates would take his wishes for commands; after Gomel, many local administrators looked upon pogroms as a patriotic and permissible weapon against revolution. During 1905-06 they played a predominantly political role as "partially spontaneous, partially organized expressions of counter-revolution" (p. 89). He makes several important points about pogroms: that the complicity of the central government is to be excluded; that some were, in fact, police riots; that even resolute local officials found it difficult to stop them; and that significant numbers of workers took part. But in the industrial centers of the interior there were no anti-Jewish riots and Löwe attributes this to the presence of a "developed proletariat with a proletarian tradition" (p. 90). No evidence is offered and other factors may have been at work—better police discipline or greater ethnic homogeneity. Ethnic tensions and demographic aspects are neglected.

This is so because the author interprets pogroms and legal discrimination, official and unofficial Judeophobia as a reactionary utopia of the agrarian, antiurban, antibourgeois, anticapitalist sectors of government and society. Russian anti-Semitism, he argues, was the accompaniment of industrial take-off, the crisis of early rather than late capitalism, an "element of a pre-modern ideal of life and society" (p. 29) in a situation of "structural heterogeneity" (p. 22), the coexistence and clash of modernity and tradition. He believes that this theoretical framework has not been used by others. The claim goes too far; no serious student could have missed the connections he makes. Nonetheless, his conception is more fully developed and more coherent than anything attempted so far. And it carries conviction.

Yet its very tidiness, the very fact that everything fits, creates unease. It arises from the author's determined use of his conceptual model and his choice of dates. One wonders how his construct would look if he had included, say, the Odessa pogrom of 1871 or even those of 1881. And the facts cited do not always support the theory. If keeping Jews out of the countryside was an anticapitalist measure, why were Jewish farmers and agricultural colonies also barred? It was not only agrarians or Slavophile utopians who wished to maintain restrictions on land purchase and residence. If land captains helped to mobilize peasants for pogroms, they did so not be-



cause they were large landowners. By 1905, most of them were ex-officers or officials of low rank. The nobility as a class and its interests are seen as too unified and their treatment by government as too consistent. Finally, after 1906, peasant land could be and was bought by nonpeasants; arguments against its passing into Jewish hands were not simply specious or designed to benefit nobles.

These are but minor flaws in a major work. But can so much irrationality and contradiction be dealt with so rationally? One wants to say what Freud said to Arnold Zweig: "With regard to anti-Semitism I don't really want to search for explanations; I feel a strong inclination to surrender to my affects in this matter . . . and my wholly non-scientific belief that mankind on the average . . . are a wretched lot." And then the historian and reviewer must quote Freud's next sentence: "Naturally, I am not reproaching you with having managed not to surrender to this irrational affect." In the present instance high praise is called for, not reproach.

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A. Z. MANFRED. *Obrazovanie russko-frantsuzskogo soiuz* [The Formation of the Franco-Russian Alliance]. Moscow: Izdatel'stvo "Nauka." 1975. Pp. 373. 1 r. 95 k.

A. Z. Manfred's *Obrazovanie russko-frantsuzskogo soiuz* is essentially a narrative of the genesis and development of the Franco-Russian alliance from 1871 to its conclusion in 1891-92. The author utilizes a plethora of relevant and useful diplomatic dispatches from the archives of the French and Russian foreign offices as well as from military and literary archives of both nations. As a narrative of one of the most significant and vital of the pre-1914 European alliances, Manfred's book contains much enlightening and useful material. His knowledge of French, German, and Russian diplomats, politicians, militarists, and their respective policies shows a profound understanding of a highly complicated and fascinating era of international diplomacy.

There are several flaws in an otherwise resourceful book. The author devotes too many passages to presenting extraneous material in support of his thesis—for example, numerous Marxist economic interpretations of the machinations and impulses of European industrialists and politicians. To Manfred, as to the Marxist historian in general, history is inexorably determined solely by economics and certain immutable laws. In reality, there are other prominent factors—psychological, social, religious—that guide and determine molders of foreign policy.

At times Manfred seems to lose sight of his thesis—the origins and development of the Franco-Russian alliance. He has a tendency to go off onto tangents regarding the personalities and policies of French and Russian journalists, Tsar Alexander III, the Paris Commune of 1871, and so on. These diversions detract from the overall manuscript, which is basically sound and scholarly.

The author presents the scholar with little new material on the formation of the Franco-Russian alliance. Moreover, much of the material taken from primary Soviet sources has already been adequately reproduced in English, French, and German scholarly works. Manfred's book is worthwhile reading material, however, for the serious scholar of the origins of the Franco-Russian alliance.

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BARBARA EVANS CLEMENTS. *Bolshevik Feminist: The Life of Aleksandra Kollontai*. Bloomington: Indiana University Press. 1979. Pp. xiii, 352. \$15.00.

Barbara Evans Clements's admiring but not uncritical biography of Aleksandra Kollontai (1872-1952), Russia's best known Marxist feminist, will be welcomed by Russianists and students of women's history alike, for Kollontai's ideas and the milieu in which she was operating are depicted clearly and objectively. Kollontai believed that socialism would eliminate conflict between motherhood and "productive work"; insisted that, although women had a duty to bear children and to breast-feed them, raising the children was society's responsibility; and envisioned communal facilities (child-care centers, dormitories, cafeterias, laundries) to assume the functions of the "bourgeois family," thereby freeing women for work and creating the material and sociological conditions for equality.

Particularly interesting is Clements's discussion of the catalytic role played by Russian feminists in forcing Marxists to pay more attention to the problems of the woman worker and her documentation of the extensive resistance, both personal and political, that Kollontai faced in her battle for equality. In 1906, for example, Kollontai managed to get party permission for a woman's meeting, only to find the room locked and on the door a sign that read, "The meeting for women only has been called off. Tomorrow a meeting for men only" (p. 46). Vera Zasulich, a veteran Marxist, told Kollontai she was wasting her time trying to organize women. Marxists, even female Marxists, opposed separate women's organizations as a threat to working-class unity, felt that women should not make special demands, and refused to examine their own prej-



udices. Kollontai's liaison with Aleksandr Shliapnikov, ten years her junior, and her marriage to the uneducated sailor Pavel Dybenko, seventeen years her junior and socially "beneath" her (she was of the aristocracy), set tongues wagging. Not even Lenin was above attacking her sex life, nor Bukharin her femininity, in order to discredit her ideas on the "Workers' Opposition" (p. 201). Demanding women's right to sexual satisfaction and the end of the "double standard," she was falsely accused of advocating promiscuity. To this day she is sometimes erroneously associated with the "glass of water" theory of sexuality, although what she really sought was a "congenial soul" who would not try to dominate her.

Kollontai's bolshevism, as distinct from her unalloyed Marxism, was ambiguous (suspicious of bolshevism in 1905, she wavered between menshevism and bolshevism until 1915), but her vision was not as "unsullied by realism" (p. 231) as Clements suggests, in effect underestimating the trenchancy of Kollontai's views. Almost alone among Marxists, Kollontai recognized the enormity of the psychological barriers to woman's equality, the habituation of men to dominance and women to submission, and the conflict this created, in women such as herself, between the need for independence and the need for love. Her advocacy in 1926 of marriage contracts and divorce insurance was ahead of its time. Her fears of bureaucracy and elitism, her fundamental distrust of power, also turned out to be justified. Openly critical of the authoritarian centralizing tendencies already evident in bolshevism even before the civil war, she resigned as People's Commissar of Social Welfare in March 1918 and ultimately sided with the "Workers' Opposition."

Focusing on the "woman question," the issue that occupied most of Kollontai's time and energy, Clements inadvertently slights other aspects of her work. One wishes for more detail on Kollontai's role in the "Workers' Opposition," for an elucidation of its relation, if any, to her work on women, and for either substantiation or refutation of John Toland's elliptical references in his biography of Hitler to Kollontai's role, as Soviet ambassador to Sweden, in the 1943 secret discussions on a separate peace between Russia and Germany. Finally, attempting, and succeeding, in writing a sober, solid, scholarly biography of Kollontai, Clements sacrifices some of the color and drama of this romantic and passionate figure; the writing is somewhat dry.

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O. N. ZNAMENSKII. *Vserossiiskoe Uchreditel'noe sobranie: Istoriia sozyva i politicheskogo krusheniia* [The All-Russian

Constituent Assembly: A History of Its Convocation and Political Downfall]. Leningrad: Izdatel'stvo "Nauka," Leningradskoe otdelenie. 1976. Pp. 362. 2 r. 40 k.

The Constituent Assembly, the most representative body elected by the peoples of Russia in their history, met only once, on January 5-6 (18-19), 1918, for a little over twelve hours. Its short life was ended early in the morning by tired Bolshevik sailors and an order of Lenin's government. The Bolshevik defense of the dissolution has traditionally been that the establishment of a republic of soviets had superseded the parliamentary stage of history represented by the assembly. In this detailed treatment of the events and conflicting views that led to that fateful January day, the Soviet historian Oleg N. Znamenskii attempts to justify at every step the actions of the Bolsheviks and in the process to discredit both the Constituent Assembly and the parties that placed their hopes on its success.

Znamenskii argues that there was never widespread or deeply rooted support for the idea of a Constituent Assembly ("parliamentary cretinism") except among "the petty bourgeois democratic intelligentsia." In 1917 workers steadily lost interest in the calling of the assembly as their hopes for an early convening were disappointed. Peasants and soldiers were more enthusiastic about the assembly, seeing it as the means by which they would at long last receive the land, but even so, the high point of interest was in the months of March and April, after which the intensifying struggle in Russian society pushed the establishment of the assembly into the background. At critical moments, particularly in the days following the Kornilov "mutiny," political leaders and their supporters renewed interest in the assembly, but much more potent was the growing allegiance to the soviets. For the author this development reflects the "overcoming by the masses of their 'constitutional' illusions" (p. 352).

Despite the author's intention to demonstrate the limited appeal of the Constituent Assembly, this reader came away with an impression of firm and consistent commitment to the assembly by most political parties and remained unconvinced from the scanty evidence presented of popular indifference to the assembly. Znamenskii notes, for example, that only slightly over 50 percent of eligible voters took part in the November elections, but, given the difficulties of organizing these elections during revolution and war and the generally higher turnout in cities, this is not a demonstration of the people's apathy. Rather it seems that Bolshevik fears that the assembly might become a rallying point for antisoviet forces had much basis in fact.

Znamenskii's study is primarily a detailed elabo-

ration of party attitudes toward the assembly through 1917, with some discussion of popular opinion, the organization of the elections, the results of the voting, and the one-day meeting itself. Yet, despite the valuable information gleaned from newspapers of the period, the picture drawn lacks clarity and definition. The intricacies and subtleties of intraparty and interparty discussions and conflicts are blurred by omissions, charged language, and the need to score polemical points. Early in the book, for example, Kamenev is condemned for his critical support of the Provisional Government in the first months of the revolution, while his close ally at the time, Stalin, is allowed to escape without a blemish. The analysis of election results is interesting but superficial, and the discussion of the assembly's meeting does not present fairly the anti-Bolshevik views of the main speakers. Though we learn much about the unrealized hopes of the moderate socialist and liberal parties and the dilemma posed by the assembly for the Bolsheviks in power, the fundamental moral-political question raised by the forcible dissolution of the democratically elected assembly remains a specter to haunt the political heirs of Lenin and bolshevized historians like Znamenskii.

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Z. L. SEREBRIAKOVA. *Oblastnye ob"edineniia Sovetov Rossii, mart 1917–dekabr' 1918* [Regional Unions of Soviets in Russia, March 1917–December 1918]. Moscow: Izdatel'stvo "Nauka." 1977. Pp. 227. 1 r. 20 k.

Soviet scholarship on the October Revolution has become a specialized art form, as highly ritualized as icon painting. Despite tantalizing references to rich archival material, Russian scholars still continue to operate within strict party directives, sometimes embroidering old themes but rarely providing new insights or approaches, though some exceptions spring to mind, like Volobuev, Burdzhakov, or Sobolev. In Soviet scholarship one misses the grand debates and the flood of journals and monographs that characterize French scholarship on their great revolution. Z. L. Serebriakova in her study of *oblast'* or regional unions of soviets unfortunately is no exception.

But Soviet historians do have access to the raw materials for scholarship and have managed to enrich our knowledge of the revolution, if not always our understanding. In particular, they have given us detailed studies of two subjects rarely treated in the West until recently—specific geographic regions and national political institutions. In this book, Serebriakova has made a significant contribution

that falls somewhere between these two themes. The thirteen regional unions of soviets were necessary links in the Soviet system between the burgeoning local soviets and the All-Russian Congress of Soviets at the top. Serebriakova helps to explain one of the great mysteries of the revolution, that is, how the Communist coup d'état in major industrial cities of the west could reach out under anarchical social conditions into the far-flung regions of Russia without losing its basic drive or direction. The regional unions of soviets took on the responsibility for coordinating the big city soviets and for filling the gaps in the countryside by creating local soviets where they did not already exist. The book is especially valuable in its accounts of the differences among the regions in 1917–18.

By the end of 1918 the regional unions had been successful in most cases and thereby created the seeds of their own destruction. Local soviets informed the People's Commissar of Internal Affairs that they wanted to deal directly with the central government. Bolsheviks at the top, like Ia. M. Sverdlov, said that the regional unions had served their purpose and now tended to swell the ranks of the Soviet bureaucracy, needlessly replicating the institutions of the central government. In the end, the new government seemed to fear that the regional soviets would develop a genuine federal autonomy that would threaten the powers of the center.

On December 23, 1918 the All-Russian Executive Committee of Soviets issued a decree that began the dismantling of the regional unions of soviets. The history of this institution seems to bear out the thesis that the civil war accelerated the tendencies in the Soviet government toward centralization and authoritarianism.

The book includes a valuable historiographic essay in the introduction and a name index, although, like most Soviet scholarly works, it does not contain a subject index.

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IRVING HOWE. *Leon Trotsky*. (Modern Masters.) New York: Viking Press. 1978. Pp. viii, 214. \$10.00.

Irving Howe was wise to write a "political essay with a narrative foundation" rather than a conventional biography of Leon Trotsky for the Modern Masters series. Trotsky's political activities and writings were so wide ranging that no one could do justice to them in a book of two hundred small pages. In any case, there already exists a three-volume biography that, despite its tendentiousness, treats the subject exhaustively. Howe's approach

enables him to describe Trotsky's main achievements and at the same time to examine critically the principal political and moral issues raised by the Communist seizure of power in 1917 and the new regime's policies in the 1920s and 1930s.

The result is a book from which the general reader can learn a good deal not only about communism in Russia but also about the attitude toward the Soviet experiment of a prominent man of the American left. Although Howe was under Trotsky's influence only briefly some four decades ago, he has remained a socialist, and, in addition to his scholarly work, he regularly and vigorously expresses his views on contemporary issues. Howe still admires Trotsky for his brilliance, versatility, and tenacity in the face of enormous adversity, and he takes great pains to present balanced, nuanced accounts of the Russian Revolution and of subsequent developments in the Soviet Union. Still, in its sum and substance the book constitutes a repudiation, from the standpoint of a democratic socialist, of Trotskyism and Leninism.

According to Howe, a critical defect of Trotsky was his failure "to scrutinize his own assumptions with the corrosive intensity he brought to those of his political opponents" (p. 135). On issue after issue he turned out to be profoundly wrong, yet he never questioned his Marxist presuppositions. For example, in his most original contribution to socialist thought—the theory of permanent revolution—Trotsky argued that because of Russia's backwardness its proletariat would be the first to stage a socialist revolution, soon to be followed by the working class in advanced, Western countries. He never could understand that a working class that enjoyed both economic gains and participation in the political process might not be interested in overthrowing the existing order. Even after the outbreak of the Second World War, Trotsky continued to speak of the need for a socialist revolution in the West, contending that only in that way could fascism be defeated. Although he had earlier urged Social Democrats and Communists in Germany to unite in a struggle against Hitler, after 1939 he claimed that workers had no stake in either side in the war, and therefore Marxists should withhold support from both. But, significantly, he maintained that should Russia be involved in a military conflict it would deserve "critical support," because the means of production in that country were still nationalized, making it more progressive than any other state.

Of course, Trotsky's fame rests largely on his accomplishments in the years from 1917 to 1922. An outstanding orator, he inspired masses of people to follow the lead of the Bolsheviks. And he almost singlehandedly created the Red Army, which triumphed in the Civil War against all odds. Relying

on the latest scholarship, Howe vividly describes Trotsky's role in shaping the Soviet state as well as his dramatic struggles with Stalin. The apt quotations from Trotsky's speeches and writings cannot fail to leave the reader with the impression that the Bolshevik leader was a man of remarkable intellectual and literary powers. But, at the same time, he clearly did much to legitimize the ruthlessness that became a central feature of bolshevism and that led to his own ultimate undoing. In this connection, Howe's thoughtful and subtle analyses of the question of the relationship between means and ends and of Trotsky's *Terrorism and Communism*, published in 1920, are especially noteworthy. The latter is a defense of terror so strident that Howe is moved to state, "If there is a single text that supports those who believe Leninism and Stalinism to be closely linked or to form a line of continuous descent, it is *Terrorism and Communism*" (p. 74).

There is an error in the work that should be corrected in future editions: the Kadet Paul Miliukov never "headed" the Provisional Government. Other than that, this is an excellent book.

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CHARLES BETTELHEIM. *Class Struggles in the USSR. Volume 2, Second Period, 1923–1930*. Translated by BRIAN PEARCE. Hassocks, Sussex: Harvester Press; distributed by Monthly Review Press, New York. 1978. Pp. 640.

Charles Bettelheim, director of the École Pratiques Hautes Études, has written many books on Marxism, socialism, and economic development. This book is a continuation of *Class Struggles in the USSR: First Phase, 1917–1923*, which appeared in English in 1977.

The author's purposes are to study "the way in which successes and failures were intermingled" in the 1923–30 period and to define "the characteristics of the social process which led to the brusque abandonment of the NEP and the changeover to the type of collectivization and industrialization that the USSR actually experienced" (pp. 11, 285). In so doing, the author outlines the development of commodity and money relations and planning; discusses agricultural policy, the transformation of social relations, and differentiation and class struggles in the countryside; describes the "contradictions" and class struggles in the industrial sector; and traces the struggle for power as seen through the changes in ideological and political relations within the Bolshevik Party.

Bettelheim's is a valuable, carefully argued, Marxist interpretation. He does see successes. Thus,

NEP, in general terms, showed the ability of the Soviet government to save the country from chaos (p. 27). Likewise, rising wages in the 1920s, even when bad for the economy, testified to the increased power of the working class (pp. 192, 243). And the "great shift" in 1928–29 brought about the defeat of the private bourgeoisie in town and country, the end to unemployment, the rapid increase in the size of the working class, the transformation of the USSR into an advanced industrial power, and the advance of socialism on a worldwide scale (pp. 322, 594).

Bettelheim, however, also feels that the great shift "weakened the dictatorship of the proletariat by causing a split in the worker-peasant alliance, starting an unprecedented [and unnecessary] crisis in agriculture, and giving rise to the development of apparatuses of coercion and repression which . . . set back socialist democracy" (p. 322). These errors came from the fundamental failure deeply to involve the working classes in decisions. Thus, Soviet planning was not truly *socialist* planning because the masses were left out (p. 290) and because the trade unions became tools to increase production instead of to change production relations. Emphasis on profitability, on one-person rule in factories, on heavy industry, on a "technicist" approach to the economy, and on decisions from above represented the re-embourgeoisement of the economy. Precisely because of the failure to change the nature of production relations, the workers did not own but were separated from the means of production (p. 304). Thus, the Soviet working class remained a proletariat, "separated from its means of production and integrated in a system of capitalist relations which have undergone only partial changes" (p. 317).

Ideologically, "the fact that a party is rooted in the working class is not enough to make it a proletarian party" (p. 331). Indeed, the Bolshevik Party became "less and less alert to initiatives and ideas coming from the masses" (p. 546). Lenin's heirs often used weak Marxist argumentation and often violated Lenin's guidelines (pp. 296–97, 448–49), began to stop analyzing their mistakes (p. 504), and developed a "simplified Marxism" that led to the adoption of policies that "were no longer based on a rigorous concrete analysis of reality" (pp. 527, 532). These errors were many. First, the party believed itself monolithic, which led to the rejection of democratic centralism (pp. 539–40), as did, second, the development of leadership that bypassed party organs (p. 459). Third, this led to the elimination of criticism and open discussion, causing even greater errors and the inability of the party to analyze and correct its errors (p. 444). Fourth, the party increasingly identified itself with the state, which led to directives from above, to the increasing use of coer-

cion, and to the belief that all opposition was from "enemies of the people" (pp. 523, 526, 545). Fifth, this led the party to believe it could *will* whatever changes it wished, which led to many errors and was, indeed, a form of philosophical idealism (pp. 519, 521, 547).

There is an infrequent tendency to ignore or rationalize certain phenomena (such as the justification of the Shakhtyi Affair or the rationalization for the indiscipline, absenteeism, plundering, larceny, and drunkenness of workers [pp. 223–24, 230]), but this is minor. The author uses a wide range of primary sources and secondary literature and draws deeply upon his own Marxist understanding. Unfortunately, the book is difficult to read: it is very repetitive and too highly compartmentalized into a chapter, subchapter, and sub-subchapter format. But it is highly recommended to any student of socialist development.

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E. I. CHAPKEVICH. *Eugenii Viktorovich Tarle*. Moscow: Izdatel'stvo "Nauka." 1977. Pp. 125. 47 k.

E. V. Tarle (1874–1955) was one of Russia's most talented, widely read, and internationally renowned historians. After 1901, when he defended his controversial master's thesis, he was entangled in scholarly political disputes, which were often regarded as indicators of relationships between political authorities and scholars. E. I. Chapkevich has previously written articles about Tarle and reveals impressive mastery of his subject's writings, of most relevant archives, and of Soviet (though not foreign) secondary literature. His main thesis is that Tarle's coming to Marxism was, despite the allegations of foreigners, a conscious act, free of compulsion.

This work exemplifies virtues we have come to expect from Soviet historians. It is the refined outcome of extensive study, discussion, and reflection. Chapkevich is intimately familiar with Tarle's works and with his entire milieu. Its defects betray the author's need for approbation by politically designated authorities. It is evident that Chapkevich knows far more about his subject than he has written. If there were not political considerations, he would not have chosen a format that excludes discussion of primary sources and historiography and also the marshaling of evidence for this thesis. Very likely, he would have provided sufficient background to reveal not just Tarle's responses to political events but also his role in higher education and in the evolution of the historical profession.

Finally, it is likely that Chapkevich would have liked to impart more information about Tarle's al-



tercations with political authorities, because they bear directly on his thesis. Tarle was arrested in 1931 and exiled for two or three years on the basis of an allegation that a counterrevolutionary conspiracy (*Prompartia*) intended to designate him Minister of Foreign Affairs in a post-Soviet government. Chapkevich takes note of changes in Tarle's understanding of Napoleon's invasion of Russia, but he treats them entirely as matters of logic. He fails thereby to place them in the context of the profound cultural changes of the period, changes that produced a redefinition of historical materialism in the 1930s, such as to accommodate it to national traditions and symbols. The attack on Tarle in 1952 in *Bol'shevik*, the Communist Party's theoretical journal, which resulted indirectly from Stalin having written in its pages about Napoleon's invasion in terms even more nationalistic than Tarle's, is left out of Chapkevich's account. His thesis regarding Tarle's Marxism is thereby discredited. This is not to affirm the contrary thesis, that Tarle's Marxism was contrived, but to suggest that Chapkevich's failure to assess such matters renders his thesis absurd. This biography is a genuinely learned and largely openminded effort. But the author's inability to invoke evidence and judge issues that encroach upon Stalinist fictions depreciates its worth.

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B. G. SAFRONOV. *Istoricheskoe mirovozzrenie R. Iu. Vipper a ego vremia* [The Historical World-View of R. Iu. Vipper and His Age]. Moscow: Izdatel'stvo Moskovskogo universiteta. 1976. Pp. 220. 1 r. 50 k.

This stimulating work, by one of Moscow University's senior professors of historical materialism, concerns the prerevolutionary career of one of the Russian Empire's leading universal historians, who today is known in the West solely for his "high Stalinist" biography of Ivan the Terrible (1942 and 1944). R. Iu. Vipper (1859-1954), who worked his way up from a job as high school art teacher to a hated post at Novorossiisk University (Odessa) and finally to a prestigious chair at Moscow University, was a prolific author of over three hundred books and articles on topics ranging from Calvin's Geneva (the only one based on extensive primary research) and Thomas More's *Utopia* to Catherine the Great and the Enlightenment, histories of classical Greece and the Roman Empire, and numerous works on the nature and method of history itself.

B. G. Safronov discusses the historical formation of his subject's outlook (1859-1900) as a radical, freethinking atheist and the influence of his major teachers: Hegelian V. I. Ger'e, founder of the Rus-

sian school of universal history and philosopher of history, who imparted his work habits to Vipper; positivist V. O. Kliuchevskii, dean of Russian history, master stylist, proponent of economic and social history and the idea of gradualism; positivist A. A. Shakhov, professor of Western European literature; and positivist classical historian P. G. Vinogradov.

The second half of the book portrays Vipper's life and works until he emigrated from the USSR (1900-23) and the evolution of his views as the major Russian exponent of empiriocriticism, expressed in a sense of crisis in his appreciation of history as a science and an increasing concern over the role of the historian in the writing of history, a movement away from analysis of and generalization about social and economic history to narrative of political history and cultural events. After reaching the summit of his career around 1905, Vipper often lapsed into careless and one-sided dogmatism (combined with flights of imaginative genius), such as manifested in the first edition of his deeply patriotic idealization of a tyrant, *Ivan Groznyi* (1923).

Safronov's work is rooted in the context of Vipper's milieu and includes extensive discussion of major issues of concern to his subject, such as the role of the individual in history, causality, "progress," historical "cycles" and "repetition"; materialism, idealism, and teleologism; the relationship between sociology and history and the natural sciences; what are "facts" and whether there are "historical laws." Equally impressive is Safronov's discussion of the leading foreign thinkers that influenced Vipper, such as Guizot, Pirenne, Thierry, Comte, Schopenhauer, Spencer, Mommsen, Lamprecht, Mach, Avenarius, Windelband, Rickert, and Adler.

The monograph is solidly based on Safronov's thorough knowledge of the period and an exhaustive study of Vipper's works and extant correspondence (he left no personal archive); student notes of his lectures, memoirs, and the records of the secret police (the Okhrana, but not the Soviet Cheka). Some readers may find a bit tedious Safronov's discussion of whether Vipper was a Marxist. The book itself is a "period piece" with its dense type and quarter-inch margins, reflecting the perpetual Soviet paper shortage.

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V. STANLEY VARDYS. *The Catholic Church, Dissent and Nationality in Soviet Lithuania*. (East European Monographs, number 43.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press. 1978. Pp. xiii, 336. \$18.00.



The Catholic Church in Lithuania has caused many headaches for Soviet leaders. As a long established institution that Soviet authority has been unable to uproot, it challenges official Soviet ideology and it also represents what Soviet leaders consider intolerable ultramontane or cosmopolitan loyalties. It has become a key rallying point for dissident Lithuanian nationalism, and Lithuanian dissidents have established a certain rapport with dissidents in other parts of the Soviet Union.

V. Stanley Vardys has produced a far-ranging study of the Church's role in Lithuanian society and in Lithuanian national consciousness. Although he emphasizes the most recent period, citing events as late as 1977, he also offers a historical survey of the traditional relationships between state, Church, and nationality in Lithuania.

The topic is a complicated one. Since the Catholic religion had been introduced into Lithuania by the Poles, the clergy was suspect in the eyes of many of the early Lithuanian nationalists in the nineteenth century. Church officials then clashed with secular authority in independent Lithuania between the two world wars. After Lithuania's incorporation into the Soviet Union, the Church had to adjust to still different conditions of existence. Vardys runs through this history in about the first third of the book and then goes on to consider the development of dissident thought in Lithuania and the publication of the *Chronicle of the Catholic Church of Lithuania*. He has drawn his information from a wide range of sources, including Soviet materials, samizdat writings, and traditional Lithuanian literature. He dates the beginning of organized dissent to 1968 (p. 128). (I remember a Lithuanian intellectual in 1970 anxiously assuring me that there was no samizdat in Vilnius.) My major complaint—apart from some technical problems of editing—concerns his decision not to discuss the Lithuanian Catholic Church under the German occupation (1941–45). He declares this period to be out of his purview (p. 292), but in other places he finds it necessary to refer to it.

Reading this book brought a number of scenes to mind. I remember in 1960 standing at a mass in Vilnius next to a woman who was reading the prayers from a text written by pencil in an address book. A Soviet Lithuanian who taught atheism once complained to me of the problems in dealing with the churches still open in Lithuania. I also remember my surprise at first seeing people kneel in the street before the icon of the Virgin Mary over the Aušros Vartai in Vilnius. The Catholic Church remains powerful in Lithuania, and Vardy's book offers a useful scholarly introduction to understanding it.

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## NEAR EAST

S. D. GOITEIN. *A Mediterranean Society: The Jewish Communities of the Arab World as Portrayed in the Documents of the Cairo Geniza*. Volume 3, *The Family*. Berkeley and Los Angeles: University of California Press, for the Gustave E. von Grunebaum Center for Near Eastern Studies, University of California, Los Angeles. 1978. Pp. xxi, 522. \$21.50.

This book—the third and penultimate volume of S. D. Goitein's masterful study of the Mediterranean Arabic-speaking Jewish communities of the high Middle Ages (tenth through thirteenth centuries) as reflected in the documents preserved in the famous Cairo Geniza—makes a major contribution to the young field of family history. To begin with, it fills a virtual void in medieval Near Eastern social history. For the succeeding period, Aḥmad 'Abd Ar-Rāziq's *La femme au temps des mamlouks en Égypte* (1973) is largely limited by the nature of its source material to upper-class women and their life within the aristocratic family. The Geniza, on the other hand, permits an unusually detailed view of the family of the nonelite classes in the medieval Islamic world. Recognizing the pioneering nature of his work, Goitein has judiciously refrained from imposing sociological models upon his evidence and has chosen instead the more appropriate method of "presenting materials" (p. viii). These mostly unpublished sources are copiously adduced, happily with a considerable amount of quotation in translation from original texts, and interpreted within the framework of problems treated by historians of the family.

The first section, "The Extended Family," explores what Goitein finds to be the dominant form of family structure. Evidence assembled includes data on patrilinear ancestor-reverence, fraternal and fraternal-sororal bonds, endogamy between cousins, and propinquity of domicile among branches of the kinship group.

The second part, "Marriage," draws mainly upon the rich deposit of marriage contracts and kindred nuptial documents found in the Geniza. Here, the materials presented pertain largely to the specific community being studied, owing to the operation of ancient Jewish law and custom. Jewish legal scholars and historians alike will relish the detail, while Islamicists will find much material for comparison. The subsection on "the economic foundations of marriage," with its extensive quantitative tables (pp. 361–422), treats such subjects of general interest as marriage gifts, dowries, and the economic role of the wife (through her earnings).

From marriage, Goitein proceeds to its corollary, "The Nuclear Family." Problems are considered under four information-packed categories: husband

and wife; parents and children (Goitein, attempting to estimate family size, finds it "quite similar to that expected in the Western world about two generations ago" [p. 240]); widowhood, divorce, and remarriage; and heirs and orphans. The materials presented here cast additional doubt upon the assumption—challenged, for instance, by Peter Laslett in *Household and Family in Past Time*—that people in the preindustrial past lived only in large, complex families and that the small, conjugal household did not emerge until early modern or modern times. In the Geniza community, a well-defined conjugal family, one in which parents cared greatly for their children and saw to their education (at least of the boys), coexisted with the extended kinship web. This finding suggests, I think, a new way of understanding the historical as well as the sociological relationship between these two familial forms.

The volume climaxes with "The World of Women," another topic in the multivalent field of family history. Though women are underrepresented in the Geniza manuscripts, Goitein succeeds in describing certain aspects of their experience. For instance, in the prevalence of the motif of dominance in the feminine onomasticon—for example, Sitt al-Nās, "Mistress over Mankind," a genre of sobriquet found also among honored Muslim ladies in the Mamluk period—Goitein sees either a "cry of protest against the oppression to which the women might have been exposed, or . . . an admonition to fight for dominance in the household and leadership in general" (pp. 316–17). In fact, the assertiveness of women appears in many areas beyond the home, particularly in economic life. The two "independent women" profiled near the end of the book—one, a poor but self-employed school mistress, the other, a "liberated" lady broker freely traversing the male domain—represent, although not the rule, at least the potentialities for women in this society. As David Herlihy suggested for the parallel phenomenon of female economic power and prestige in early medieval Europe ("Land, Family, and Women in Continental Europe, 701–1200," *Traditio*, 18 [1962]: 89–120), the extreme mobility of the men would seem to have been an important factor creating the circumstances in which this could happen.

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J. E. PETERSON. *Oman in the Twentieth Century: Political Foundations of an Emerging State*. New York: Barnes and Noble. 1978. Pp. 286. \$23.50.

The recent emergence of the Persian Gulf as a region of pivotal importance in world affairs has stim-

ulated a plethora of analyses, articles, and books. Although the quality of much of this work is marginal, J. E. Peterson's history of modern Oman deserves serious, albeit cautious, attention.

In essence, Peterson attempts two tasks. First, he explains four major themes that he believes have conditioned the political evolution of twentieth-century Oman; these are identified as the interaction among the sultan and the ruling family, the development of administration, the functioning of tribal politics, and the influence of external forces—especially those represented by Britain. Second, he provides a politically oriented analysis of the country's modern history. Organizationally, the work consists of (1) a short introductory essay that offers a general geographic and cultural perspective, (2) four topically arranged chapters concentrating upon the major political themes, (3) four chronologically ordered chapters focusing upon the fortunes of the Omani Sultanate since its near nadir in the 1890s to Sultan Qabūs's modernizing regime that was installed by the 1970 coup d'état, and (4) a useful collection of maps, genealogies, and other appendices.

The book makes a genuine contribution. It is solidly based on a variety of sources including recently opened British archives that provide essential information on Omani events through the late 1940s. It fills large gaps in our knowledge of Oman between 1913 and the 1950s, especially of political and administrative developments within the sultanate. It treats the several military campaigns that mark recent Omani history, such as the recently concluded Dhufar hostilities, very effectively.

Nevertheless, while the volume enlarges our factual knowledge, I believe its interpretation of recent Omani history—especially of the last thirty years—is inadequate. To me, the book is too concerned with the facade of political events and does not really come to grips with the complex web of intellectual, social, and economic currents that are inseparably bound to the evolving political situation. The implicit assumption that the secularly oriented institution of the sultanate should be considered to be Oman's political norm is the most disturbing result of this preoccupation. Indeed, the essence of much of Oman's recent history is missed when movements indicating persisting support for an alternative ideal for ordering Omani society, the theologically based imamate, seem to be dismissed as so many "rebellions" against the sultanate. Finally, the effect of the author's energetic mobilization of secondary and oral sources to provide new information is dissipated because he seems unwilling to develop original interpretations of Oman's last thirty years.

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ROSEMARIE SAID ZAHLAN. *The Origins of the United Arab Emirates: A Political and Social History of the Trucial States*. New York: St. Martin's Press. 1978. Pp. xix, 278. \$22.95.

In late 1971, amidst drastic economic, technical, and social transformation, seven sheikhdoms on the lower Persian Gulf attained political independence from Britain as the United Arab Emirates. This new nation's territory, known to Arabians as the Oman Coast, but to British officialdom as the Trucial Oman, the Trucial Coast, or the Trucial States, had remained in a precarious condition of tribal feuding with a bare-subsistence economy from 1820, when Britain imposed the first in a series of maritime treaties and truces, until after World War II.

Rosemarie Said Zahlan's book brings together for the first time detailed information about internal political developments on the Oman Coast from 1919 through 1939—years that she rightly designates as a "vital and neglected period in their history" (p. xiii). She bases her account mainly on documents (partly in English and partly in Arabic) of the India Office Records in London, indicating that the most valuable for her purposes were the recently opened Persian Gulf Territories Residency records.

The author pursues some half dozen topics from the period under study and either states or implies some unsurprising conclusions. For instance, she sees each ruler depending largely on his own skill in maintaining support within his family and tribe and in defending his tribe's interests against those of neighboring factions. The role of British supervision in the region varied according to imperial interests and the personalities of the officials, who at any given time numbered only a handful but who were upheld by the implied threat of a gunboat. Britain played an effective part in warding off the encroachments of Iran and especially rapidly expanding Saudi power.

The account intimates British violation of the principle of noninterference in the acquisition of air bases on the route to India. Aspects of interstate relations were altered at the end of the period with attempts to delineate permanent territorial boundaries to meet the requirements of oil concessions. Control of the rulers' foreign affairs was used to assure that the concessions went to interests agreeable to Britain. The volume has detailed documentation, a useful map, a comprehensive bibliography, genealogical tables of ruling families, lists of British officials, seventeen illustrations, and a good index.

The author's analysis is careful and cautious, although she is perhaps too reticent about making generalizations. Some readers may miss any specu-

lation on such intriguing questions as the extent to which Britain's presence was responsible for the survival of these states independent of Saudi Arabia.

The idea behind the title of the book may be found in the assertion that during the interwar period the sheikhdoms "began to acquire their present character and their present enormous significance in world affairs" (p. 180). The reviewer would insist that to at least an equal degree—and in terms of social history (scarcely touched in the book) in a much greater measure—the origins of the United Arab Emirates lie in the crescendo of wealth that began in 1962. This has produced a growing flood of goods virtually unknown previously and waves of foreign immigrants arriving to supply both the menial and the technical services that have transformed the entire way of life in the country.

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SAFIUDDIN JOARDER. *Syria under the French Mandate: The Early Phase, 1920–27*. (Asiatic Society of Bangladesh Publications, number 31.) Dacca: The Society. 1977. Pp. xii, 241. Tk. 45.00.

Twenty years ago Stephen H. Longrigg concluded the preface to his history of the French mandates in Syria and Lebanon by looking forward to the day when some future historian would retell the story "with greater fullness and more penetration." Unfortunately, Safiuddin Joarder's book falls short of this anticipation. The book is a slightly revised 1967 dissertation that ignores much published material and archival resources made available since then.

Joarder focuses on the first years of the French mandate in Syria. He scrupulously outlines the economic, political, and strategic difficulties facing the mandatory power but concludes that French policy (rigid centralization conducted by ill-chosen military officers, failure to cooperate with urban elites, the "morselization" of Syria into nonviable administrative units) served to embitter Syrian nationalists. The resultant intransigence boiled over into a general national rebellion against the French from 1925 to 1927, indelibly tainting future Franco-Syrian relations. In general, the thesis is unexceptionable, although Joarder's effort to draw attention to the Syrian nationalists would carry added weight had he cast his net more widely. French insensitivity was merely a last step in a series of frustrations for Syrian nationalists, capped by the post-World War I decision to divide "geographical Syria" into several truncated mandates.

Joarder is on less certain ground when he claims (p. 194) that the 1925–27 uprising had little to do with future constitutional developments. Such a

judgment may be forced by the fact that he ends his book abruptly in 1927, thus foregoing a systematic analysis of the aftermath. Others have suggested rather convincingly that precisely because the Druze rebellion of 1925 assumed national proportions, it compelled the French finally to work more energetically toward fulfilling mandatory requirements for a constitutional statute.

Where Joarder's study has value is in its use of Arabic sources. He has exploited Arabic memoirs, diaries, and secondary works more thoroughly than other scholars writing on this subject in English. These materials help illuminate the social character of those Syrians who participated, either actively or passively, in the rebellion of the mid-1920s. He has also used League of Nations reports effectively. The usefulness of the study would be enhanced, however, had the author supplemented his heavy reliance on U.S. documents with a thorough consultation of British and French sources—especially the French foreign ministry and army archival materials, available since 1972.

Finally, a word must be said about the book's haphazard composition, which the author laments in the preface. Misused words, and spelling, grammatical, and typographical errors abound on nearly every page. Publication should have been delayed until these and other matters were corrected.

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FARUK SÜMER. *Safevi Devletinin Kuruluşu ve Geliş mesinde Anadolu Türklerinin Rolü. Şah İsmail ile Halefleri ve Anadolu Türkleri* [The Role of the Anatolian Turks in the Establishment and Development of the Safavid State]. (Selçuklu Tarih ve Medeniyeti Enstitüsü Yayınlar, Tarih Dizi, number 2.) Ankara: Güven Matbaası. 1976.

The author comes to seven conclusions, all of which are worthy of discussion, some of debate, and some excitingly new. The rise of the Safavids is an interesting and exciting historical problem that is in need of further study, especially extended monographs. It is coincidental but appropriate that I am writing this review one week after the revolution of Ayatollah Ruhollah Khomeini in Iran, an event that some scholars have already suggested is one of the greatest movements in Middle Eastern history since the creation of the Safavid state in 1501.

Faruk Sümer is a noted scholar and perhaps the most noted authority on the history of the early Turkic tribes and confederations. In this book he brings to bear his immense erudition and knowl-

edge on the role of the Turkish tribes in the creation of the Safavid state. Not surprisingly, and in brilliant fashion, he elucidates that role.

Sümer's work has seven conclusions, all of which partially or completely revise the works of previous scholars such as Vladimir Minorsky, Walter Hinz, Mükrimin Yınaç, I. P. Petrushevsky, Claude Cahen, Osman Turan, Jean Aubin, Michel Mazzaoui, Yaşar Yücel, and Hanna Sohrweide as well as the most recent work of John Woods. His conclusions fall largely into four general categories: (1) the role of the central Anatolian (and emphasis must be put on the word central) Turks was an entirely new factor in the origin of the Safavids; (2) the tribes that belonged to the Kara-koyunlu and Ak-koyunlu confederations only entered the service of the Safavid state after it had been established; (3) the shamanist religious beliefs of the Anatolian tribes, which stressed great reverence for their shamans—dedes, atas, tabibs, and so on—played an important role in their acceptance of Safavid emissaries and in increasing the influence of Twelver Shi'ism especially after 1316; and (4) in addition to fundamental political and social cleavages, ethnic and linguistic differences, especially literary emphasis, also existed between Turks and Taciks (Iranians). According to Sümer, "rather than being a successor of the Ak-koyunlu state it would not be out of place to consider the Safavid state as a revival of the İlhanlı (Il-Khanids) empire." Henceforth, all works on the origins of the Safavids will have to consider the evidence offered by Sümer in this exciting book.

When I reviewed John Woods's book on the Ak-koyunlu (*Middle East Journal* [1977], 367), I stated that perhaps it would greatly clarify the issues involved in the origins of Ak-koyunlu and Safavids if scholars simply termed the events surrounding these origins as "Turko" politics and then defined that term rather than the usual "Turko-Irano" politics that simply through usage has achieved a single adjectival meaning despite the hyphen. The Arabists dominated the Orientalist market up to the 1950s, and it has been only in the last twenty-five years or so that the "Perso" influence has achieved its proper place in the field of Middle East history. One suspects that the Islamo-Persianists with so recently an acquired position in "Orientalia" bridle at the term "Turko" to describe the "Turko-Perso-Islamic" political and social structures that obtained during this period. Whatever the case, Sümer goes a long way in granting parity to the "Turko" portion of that triad. His work facilitates the further demise of the idea that medieval and early modern Middle Eastern history was largely an Arab and Persian affair.

ROBERT OLSON  
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HALIL IBRAHIM SALIH. *Cyprus: The Impact of Diverse Nationalism on a State*. University: University of Alabama Press. 1978. Pp. x, 203. \$13.50.

No scientific research in any field, be it history or physics, is complete if it does not present a hypothesis that must be proved or disproved by research. Research without a hypothesis is mere description—it does not explain. Halil Ibrahim Salih's thesis is that the Greek and Turkish communities in Cyprus cannot form a strong federation. In his view, the only realistic solution is division (*taksim*), either through "double union" (Clerides's stand) or a very loose federation (Denktas's stand). The author's earlier book, *Cyprus: An Analysis of Cypriot Political Discord* (1968), of which the present work is a 1975 updated version, presents the same hypothesis.

The present book is arranged to prove this hypothesis. In the first chapter, "Historical Background," none of the elements of Greek-Turkish solidarity since Cyprus's inclusion into the Ottoman Empire in 1571 is mentioned. For instance, the Greek revolt of 1931 against British colonial rule was supported by the Turkish community. The Turkish press also condemned British repression of this revolt. But these facts are ignored in this chapter, while elements of Greek-Turkish division are stressed. Traditional oversimplifications are unfortunately used, such as "Greeks and Turks have distrusted one another for hundreds of years" (p. 87).

In the second and third chapters on the "Roots of the Crisis" and the "Sources of Cypriot Disunity," Salih is impartial in his description, blaming neither the Greek nor the Turkish side. But just by pointing out such disunity, he forces the reader to conclude that no understanding between the two communities has ever been possible. For instance, he implies that the 1960 constitution of Cyprus was a priori unworkable. Probably for fear of being accused of not being impartial, the author does not adequately underline the personal responsibility of Makarios for such a failure.

Salih points out and proves that there is no such thing as a Cypriot nation and that the loyalty of the two communities goes to their respective motherlands, Greece and Turkey (p. 25). But again, just by being objective on this point, he implies that the normal solution is the division of the island. Classical Western Renaissance logic (a distortion of Greek thought) is divisive. Marxism was an attempt to rejuvenate and improve dialectical thought found in non-Western civilizations such as China and Greece. Classical logic says that, on an issue where one party is right, the opposite party must be wrong. Dialectically speaking, however, the Cyprus conflict is a clash of right against right. The proper solution cannot, therefore, be divisive, as this book

advocates. It must be a higher synthesis qualitatively different from its components. Such a synthesis could be a confederation of two sovereign states (Athens and Ankara) including Cyprus.

The two major events in the history of independent Cyprus, the civil strife of December 1963 and the Turkish invasion of the island in 1974, are also presented in an impartial way in chapters four and seven. This certainly makes the book commendable, but since it was only updated in 1975, the events of 1974 were still too close to the time of the author's research to allow a deeper analysis. The bibliography is relatively poor—most of the relevant French and Greek titles are not mentioned.

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## AFRICA

J. D. FAGE. *A History of Africa*. (The History of Human Society.) New York: Alfred A. Knopf. 1978. Pp. x, 534. \$17.95.

J. D. Fage is a household name among African historians. He has published a number of textbooks about African history, edited the *Journal of African History* with R. Oliver from 1960 to 1973 and, also with Oliver, edited the *Cambridge History of Africa* since 1975. He was the first professional historian to teach African history, which he has been doing since 1949. Today he directs the Centre of West African Studies at the University of Birmingham. This book is his first solo "History of Africa," but he knows the genre and the requirements textbooks must meet.

This work of nineteen chapters is organized in four thematic sections. The themes are trade and state, Islam, European expansion, and the colonial experience. There are no strict chronological divisions. Thus the section about the impact of Islam includes developments in nineteenth-century West Africa, while European activity there is included under European expansion, more than one hundred pages later. The colonial period is given 102 pages, one-fifth of the total, which no doubt reflects the author's conviction about its proper importance in the long sweep of history. By area the continent is divided into North, West, and the rest. Inevitably the rest often is slighted. No explanation is offered to justify why the whole continent is included. It is just the trend of the times, as B. Lewis noted in his *History—Remembered, Recovered, Invented* (1975). (Lewis, being a historian of the Middle East, cannot accept the loss of North Africa, especially Egypt, nor can he accept a permanent overlap of this area in the name of continental ideology.)



Fage's view of Africa's past deals with "state and trade," the Whig interpretation of African history that developed in the 1960s. It is not much of an oversimplification to state that, for him, historical development consists of the adoption of agriculture, the creation of surplus, overpopulation resulting from that, migration, concomitant growth of trade, followed by the appearance of states and their expansion. Europe's expansion into Africa is part of the process—an anemic view of history to my mind. Where are the feelings, the aspirations, the pleasures, the intellectual speculations, the artistic expressions of past African societies? Where is the history of African cultures? It is a pity that the author never could undertake fieldwork in a rural area in Africa. If he had, his history might have been very different.

Critics of the Whig view of history, as it was in the 1960s, have not impressed him. The reader should be warned that this book represents a personal point of view, which leaves several trends in recent historiography unmentioned, such as Marxist interpretations, dependency theory, and approaches to "global" history. The bibliography is infuriating precisely because it is a personal statement. Although the list of journals is acceptable, the eyebrow is raised when one sees that only articles from the *Journal of African History* are mentioned, except when it comes to West Africa where journals from Nigeria and Ghana are also found useful. There is a certain parochialism about it all and this book clearly is a product of what some have dubbed "The School for Oriental and African Studies School."

While this characteristic makes for weaknesses—and any historian of Africa who reads the book will find many, especially outside of West Africa—it also gives a unity of thought to the work. I enjoyed learning Fage's view about Europeans in Africa in the nineteenth and twentieth centuries and found his discussion of the slave trade very clear. A volume of essays might have been a better genre to express these opinions than a general history, for, appearances notwithstanding, this is not a textbook at all. It is less and it is more.

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L. H. GANN and PETER DUIGNAN. *The Rulers of British Africa, 1870-1914*. (Hoover Institution Publications.) Stanford: Stanford University Press. 1978. Pp. xi, 406. \$18.50.

This wide-ranging study deals with the whole of British sub-Saharan Africa from the years before the Scramble to the outbreak of war in 1914, focus-

ing on the work of soldiers and administrators, both European and African, and of professionals such as doctors, teachers, agronomists, and engineers. It aims to provide a comprehensive survey of the running of British Africa by looking at the personnel, spirit, methods, and instrumentalities of the ruling power. It evaluates performance as it goes along and passes judgment at the end. Continuing in the tradition of their excellent and courageous *Burden of Empire* (1967), the authors again identify themselves with the under-manned school of realism in colonial studies, shunning both the racism of extreme Eurocentricity and the shrill political pamphleteering of early nationalism.

Essentially an essay, the book draws mainly on secondary literature, though there is a sprinkling of references to archival materials. There are many important compilations and statistical tables, which add greatly to the value of the work as a reference for students undertaking studies on discrete topics. Perhaps the next volume will include a bibliography, either by chapters or arranged topically at the back. In addition to being helpful to those wishing to do further reading, this would show that the authors have in fact examined large numbers of relevant books not found in the notes.

Gann and Duignan are at their familiar best when speaking on a high level of generalization about African societies and economies, about England's preoccupations in late Victorian times, and about problems of race relations as white met black in the confused, experimental pioneer time. How right they are to talk of administrators as gifted amateurs and of soldiers as gentlemen first and specialists second. The chapters on communications, medicine, education, and agricultural reform are skillfully organized, clearly set out, and wonderfully illuminating in their presentation of complex developments across the face of a varied continent through periods of turbulence and change. The scope is vast and would be daunting to scholars of less experience and smaller resource. Their treatment moves all the way from British society and government to the particulars of colonial office machinery, to governors in Africa, armies in the field, British and African officers at work, and all the substructures of administrations that covered a wide field of activity, even in these early days.

Old hands will find little things to pick at and fuss over now and again. A glance at colonial office minutes, where the guts of its doings are recorded, would give a more accurate picture than we get here and would allow the authors, for example, to relate Lugard's complaint about meddling to accounts of his own role in the office through the years, especially in selecting governors, a process much more heavily influenced by retired ex-

cellencies than is usually realized. The concern with development had a full head of steam long before Ommanney's time. The paper unity of the colonial services in 1896 had little reference to Africa. The junior from the bush who got in a word with Joseph Chamberlain was a rarity. Governors had prestige, all right, but their influence cannot be measured without tracing the work of representative operatives all the way down to the sand. Belfield is described as unmilitary. Why should it be otherwise with a lawyer from Rugby and Oriel who had completed thirty years of purely administrative slogging? By 1901, we are told, none of the top men were from army backgrounds. What about Lugard, and a few years later, Girouard? In citing Kipling's castigations of public schools should the authors not tell us the personal reasons for his feelings, noting at the same time that the average civil servant had very different boyhood and school experiences from his? The most common reason for going into colonial service was the need for a job, a pedestrian motive hardly noticed here.

Rather a lot of attention is given to theorists and singers—Marx, Belloc, Kipling, Wilde, Newbolt—and less than one would like to the men themselves, their lives, their work, their thoughts, and the impressions they made on Africans. Perhaps this is because the authors are often at several removes from their subject, dependent on what somebody else has said. There is an abstract, derived quality to parts of the chapters on district officers and governors. Had they gone deeper into private and official papers, they might not be so didactic about such matters as the right sources to consult on public schools in England and about the incidence of homosexuality in Africa.

A subject as complicated as imperial rule is better approached from the bottom up than from the top down. The book repeatedly strays from its time period and from Africa, which might have been less likely if the light had been fixed directly and consistently on the principal actors and their stories. The authors continually put forward examples that purport to be representative but that end by illustrating the difficulties of generalization in so diverse a field.

None of which denies the importance and value of this latest contribution from two eminent scholars who remain in the forefront of imperialism's interpreters. Once again they have placed all of us deeply in their debt.

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GERALD W. HARTWIG and K. DAVID PATTERSON, editors.  
*Disease in African History: An Introductory Survey and*

*Case Studies.* (Duke University Center for Commonwealth and Comparative Studies, number 44.) Durham, N.C.: Duke University Press. 1978. xiv, 258. \$13.75.

Historians of Africa have only just begun to comprehend the enormous importance that disease has had in the economy and well-being of Africans throughout history. According to Gerald W. Hartwig and K. David Patterson, editors of *Disease in African History*, the neglect of disease in precolonial and colonial history leads to an incomplete projection of social, political, and economic events. They hold that the impact of ecology, epidemics, and malnutrition needs thorough investigation. Historians of non-African areas have recently drawn attention to disease and its impact on social stability, economic growth, and demographic change. The authors wonder why in African history disease has not attracted similar attention. Like other societies, Africans were compelled to come to terms with pathogens that mobilized their body's defenses. Even in the beginning, when the hunting stage was replaced by a more static mode of village life, biological and social adjustments were apparently made successfully. More dramatic, however, has been the impact of the tropical environment in sub-Saharan Africa and the break-up of relative isolation in precolonial times, especially in response to trading caravans from without and African participation in trade thereafter. These and other factors, it is argued, led to periodic interruptions of an established biological and ecological balance (pp. 6–7).

It is the intention of this book to use the untapped resources of medicine, geography, and demography in order to rewrite African history from a sociological and disease-oriented perspective. The authors propose a holistic approach that would take into account "agriculture, economics, housing, settlement patterns, demographic composition, social organization, cultural and religious values, medical technology, political conditions, epidemiological history, nutrition, climate, vegetation, fauna, topography and other factors . . ." (p. 20). Their views are summarized in the first chapter, an introductory overview of the disease factor. They are substantiated in seven articles by different authors who range over a variety of cultural and geographical topics concerned with epidemic and endemic diseases. They cover specific diseases like tick-borne relapsing fever in East Africa (Charles M. Good), river blindness in Northern Ghana (K. David Patterson), louse-borne relapsing fever in the Sudan (Gerald W. Hartwig), and groups of diseases that afflicted large areas such as early colonial Ashanti (James W. Brown), Cameroon (Mark W. De-

laney), and southern Chad (Mario Joaquim Azevedo). A concluding bibliographical essay by Patterson gives the reader a wide range of literature. It includes medical history in general and works on history and disease outside Africa. The section on disease and medicine in Africa is more extensive and opinionated. For precolonial Africa it selects medical anthropology and traditional medicine. It lists in much greater detail the vast literature on the colonial period according to regions. The bibliography will help the beginner find his way but it does not even hint where to find the original materials that are so important for the kind of historical writing the authors advocate.

A review of a multi-author volume is restricted to a mere sampling. Therefore only a few examples can be given here. In "Social Consequences of Epidemic Disease: The Nineteenth Century in Africa," Hartwig attributes social and demographic changes among the Tanzanian Kerebe to epidemics of cholera and smallpox in the earlier part of the nineteenth century. They affected the nuclear family by causing the absorption of immigrants as servants and by necessitating increased reliance on adoption. His field studies on the Kerebe supply new data for his interpretation of the causes of demographic change precipitated by epidemics. There are also interesting comments on the emergence of sorcery in the wake of sudden deaths following epidemics.

Charles M. Good's article, "Man, Milieu, and the Disease Factor: Tickborne Relapsing Fever in East Africa," analyzes the success or failure of officials to react expeditiously to relapsing fever, which caused much suffering and economic distress. Many factors are cited that hindered control of the fever, such as migration to stricken areas, the use of huts with materials favorable to tick propagation, and reaction by Africans to controls imposed by authorities. Maps, statistics, and charts accompany Good's evaluation.

Patterson's study, "River Blindness in Northern Ghana, 1900-50," deals with the frustrating question why the discovery of the cause of river blindness came only in the late 1940s although the disease incapacitated 3 percent of the population of Northern Ghana. In spite of the progress of medicine and scientific research after World War I, medical advance did not always have an immediate impact on the colonies where doctors and junior officials were limited in numbers and where money was hard to come by.

The importance of this book lies in its attempt to focus attention on the role of disease in African history. The authors' presumption, however, that they are presenting a set of problems hardly seen or examined before by historians is unwarranted. Even early explorers of Africa saw connections between

climate, agriculture, and disease in areas through which they traveled without quite understanding them.

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RENZO DE FELICE. *Ebrei in un paese arabo: Gli ebrei nella Libia contemporanea tra colonialismo, nazionalismo arabo e sionismo (1835-1970)*. (Nuova collana storica.) Bologna: Il Mulino. 1978. Pp. 464. L. 12,000.

The author of this book is a well-known Italian historian whose works on Mussolini have provided new insights and generated controversy. One side issue of fascism is the attitude of the Italian government toward the Jews in Italy and in the colonies. Renzo de Felice's *Storia degli Ebrei Italiani sotto il Fascismo* (1972) describes the measures taken by the Fascist government to solve the Jewish "problem" in Italy. In the second volume, *Ebrei in un paese arabo*, the author concentrates on the two-thousand-year presence of the Hebrew community in North Africa, its relationship with the Arabs, the impact of Italian colonialism, the Second World War, the reaction of Arab nationalists to the Jews of Libya, and the Arab attitude toward the Jews during the Arab-Israeli wars.

In the precolonial period, Jewish relations with the Arabs were generally good even though on occasion, because of a local case of religious fanaticism, the Jewish population was discriminated against politically. During the colonial period, in spite of Jewish suspicions of the real motives of the new rulers, the Italian administration improved the standards of living of the Jewish community; it also respected their civil liberties, culture, and traditions. The policy of the liberal Italo Balbo, governor of Libya, was to Italianize the Jews and make them metropolitan citizens.

With the British occupation of Libya in 1943, the Jewish population suffered two pogroms. Although the Jews accused the British military administration of inciting the Arabs, available evidence supports only the conclusion that the British were slow and inefficient in preventing the massacre for lack of well-trained and loyal native police. Because of the lack of protection of the Jewish community, Libyan Jews emigrated *en masse* when Israel was created as a state in 1948. Those who remained behind under the increasingly nationalistic and anti-Zionist revolutionary government of Gheddafi abandoned the country in 1967, forfeiting property estimated at \$400 million. The year 1970 saw the end of the two thousand years of Jewish presence in Libya.

De Felice's research on Judaic history in North Africa since the mid-nineteenth century is a clear

and chronological narrative of the sociopolitical, psychological, economic, and religious conditions of the Israelites. The author points out that the best period of Jewish history in Libya was during the Italian administration. He also shows that in various political situations Arab-Jewish relations were good, which in itself reveals positive Arab-Jewish relations at a time when the Arab world is divided over whether to reach a compromise with the state of Israel. Another important aspect of De Felice's work is its encyclopedic wealth of detail and the evaluations of specific points of contention. His major contribution, however, is his analysis of the state of mind of the Hebrew people in a hostile environment. The thrust of the book is that Jews and Arabs can live together because culturally they have much in common, and there is the inference that peaceful coexistence among these Semitic cousins has been prevented by political events and fanatical or weak leaders who were not able to live up to their mandate to rule wisely. De Felice's history of the Jews in Libya is a major historical contribution that other historians should emulate to produce definite evidence of the relations between Jews and Arabs in other Arab states.

The book is the result of research conducted in the archives of Jewish international organizations in the United States, Israel, Great Britain, and Italy, as well as in European diplomatic documents. The author also scrutinized the personal papers of leading personalities and took oral testimony from prominent eye witnesses. This book will be appreciated by both scholar and layman, and it is an indispensable source of information libraries will not want to do without.

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SVEN RUBENSON. *The Survival of Ethiopian Independence*. Reprint with corrections. (Lund Studies in International History, number 7.) New York: Africana Publishing. 1978. Pp. 437. Cloth \$30.00, paper \$20.00.

The single most outstanding contribution to the historiography of modern Ethiopia, Sven Rubenson's "Some Aspects of the Survival of Ethiopian Independence in the Period of the Scramble for Africa," has for nearly twenty years lain inaccessible among the 1960 Leverhulme conference papers published in Salisbury, Rhodesia. Here at last is the detailed sequel, which amounts to a history of Ethiopia's foreign relations from the 1804 Valentia mission until the battle of Adwa in 1896. The great strength of Rubenson's approach is that this is throughout a history from the Ethiopian side. European sources

are analyzed from the viewpoint of Ethiopian rulers, whose own original letters receive special attention, enabling him to strip off from their attitudes and intentions the layers of deceit and delusion with which European emissaries habitually covered them. His grasp of the sources, both European and Amharic, his profound knowledge of the period, and his uninhibited readiness to appraise, argue, and judge make this the work of a master.

The book is, however, more a general diplomatic history than an answer to the question implied in the title. The period before 1855 occupies more space (143 pages) than the better-known reigns of Tewodros (116 pages) and Yohannis and Minilik together (119 pages), though it is a story largely of attempts by ambitious intermediaries to boost their own importance despite the indifference both of Ethiopian rulers and of their own governments. From the accession of Tewodros, it becomes possible to identify an Ethiopian foreign policy directed toward consistent objectives—international recognition, maintenance of historic frontiers, access to the sea, import of foreign technology—and pursued for the most part with considerable restraint and by diplomatic means, despite the constant duplicity of European powers that induced in Ethiopia's rulers a wholesome determination to trust only to their own resources.

The particular problem of diplomatic history is that it deals with the contacts between states whose internal workings, on which diplomacy rests, are unexplored. Rubenson is constantly aware of the internal dimension, most effectively in his treatment of Tewodros, and he can draw on several existing histories and biographies of leading participants. Nonetheless, gaps remain, most seriously on the topic that should attract most attention, the survival of Ethiopian independence. Though this owed much to skillful diplomacy, it also depended on drastic changes in domestic political structure, on the economic resources captured by territorial expansion, and in the final period on arms imports—all of which are noted but not examined. One is aware, too, of increasing haste as the book nears its end, and it closes with Adwa in 1896, rather than with the subsequent treaties and frontier demarcations with which independence was assured, even though Eritrea was lost. Since in the earlier confrontation with Egypt, Ethiopia won the war yet lost the peace, and since the frontier demarcations were to have momentous consequences, these agreements are worthy of scrutiny. In restricting itself to pre-1896 diplomacy, admirable though this book is, it leaves the promise of the earlier paper partially unfulfilled.

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FRANÇOIS RENAULT. *Libération d'esclaves et nouvelle servitude: Les rachats de captifs africains pour le compte des colonies françaises après l'abolition de l'esclavage*. Abidjan: Nouvelles Éditions Africaines. 1976. Pp. 239.

After abolishing slavery in their colonies in the nineteenth century, European nations attempted to cushion their colonial producers from economic disruption by providing them with a dependable source of inexpensive labor. Several scholars have studied the process whereby Great Britain accomplished these aims by establishing "free black" emigration from Africa after 1840, but researchers have largely neglected France's efforts along these lines after the Revolution of 1848 brought emancipation for French colonial slaves. François Renault has now provided historians with a thorough study of France's practice of recruiting laborers by *rachat préalable*, purchasing them on the African coast, redeeming them from slavery, and transporting them forthwith to Réunion, Guadeloupe, Martinique, and French Guiana.

Renault briefly describes the situation in France's colonies after 1848 and the decline in production caused by emancipation before going on to discuss the dispositions taken by the French government as of 1852 to recruit plantation workers. Failing in its attempts to obtain an adequate supply of European contract laborers, Indian coolies, and free Africans, France authorized shippers in 1856 to purchase blacks from slavery on the East and West Coast of Africa and transport them to France's tropical possessions. In his examination of the enrollment and shipping of "redeemed slaves" the author systematically discloses the marked similarities between this method of African emigration and the slave trade: blacks were purchased from slave dealers; they were detained in squalid barracoons on the coast prior to embarkation; they were transported in the filthy, ill-ventilated, overcrowded holds of ships; they frequently attempted mutiny; they suffered relatively high mortality rates during passage. Indeed, Renault shows that France's system of emigration by purchase from slavery was nothing but a "disguised slave trade" designed to provide colonial establishments with an indispensable supply of labor. France finally terminated its recruitment of redeemed slaves on the West Coast of Africa in the early 1860s, when an Anglo-French agreement permitted French colonies to import Indian coolies, but some French traders on the East African Coast surreptitiously continued to purchase from slavery a small number of black emigrants until the last decade of the nineteenth century. Between the 1850s and the 1890s more than sixty thousand Africans were forcibly transplanted to France's colonial possessions by the practice of *rachat préalable*.

Renault's findings confirm what scholars familiar with the subject have always surmised about France's African emigration projects after 1852. His study, followed by a series of documents related to the topic, is well researched in the Section Outre-Mer of the French National Archives. Written in a simple, readable style, it tends to be somewhat wordy and redundant at times; but many historians will undoubtedly appreciate the huge amount of factual data it provides on all aspects of the French system of recruitment by redemption.

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THURSTAN SHAW. *Nigeria: Its Archaeology and Early History*. New York: Thames and Hudson. 1978. Pp. 216. \$16.95.

Thurstan Shaw's career has spanned the period that saw the professionalization of West African archaeology, a process to which he has contributed more than any other person as the exemplar in sensitive excavating and publishing full and precise reports on the sites, in teaching and preparing Ibadan University students to become archaeologists, and in educating the public, locally and generally, through lectures and writings on the value and meaning of his discipline and its findings.

In this book he summarizes some of his work (he has also excavated in Ghana) and that of other archaeologists who have worked in Nigeria. Fagg's work on Nok, Willet's on Ife, Connak's on Benin and that of others are drawn together in a regional historical framework that also reaches out and sets the Nigerian development in a larger African and global setting. The Old Stone Age, Neolithic, and Metal Age sequence is elucidated, but the story is not just one of technology and radiocarbon chronology. Shaw apparently agrees with James Mellaart that "archeology is concerned with the human beings in the past and therefore such a study is clearly part of the humanities" (*The Neolithic of the Near East* [1975] p. 10). Attempts are made, where feasible, to relate early materials to communities that can be identified in terms of language and to tackle such problems as religion in the given period.

The last part of the title poses a problem: from one point of view archeology is history and therefore the juxtaposition is redundant; but, if the copulative indicates that they are to be considered supplementary but distinct, then the second is not readily perceived. The "early history" is from a wholly archeological perspective: what has been revealed from the earth is dealt with, but areas awaiting excavation are given short shrift. Nupe, Idah, Jukun, Bornu, the Hausa states, the "middle belt"



peoples, the mangrove dwellers, and the eastern-most fringe of the country are given, at most, occasional passing mention. They do not hold the stage to the extent that Igbo-Ukwo does, although some of them were certainly as significant, but, of course, the author is aware of this (see p. 188). Each approach to history writing has its emphases and oversights and the reader should be aware of such tendencies to skew any account. This quibble aside, historians will be happy to have a summation of the current archeological data in the interpretation of such an eminent participant in the ongoing search for the past of Nigeria's peoples.

Art historians will also appreciate this book for its numerous fine illustrations of bronze, terra cotta, and stone sculptures. Ninety-odd depictions of art objects make up about two-thirds of the illustrations of the book; the others are implements, excavation plans, maps, and so on. The prominence of sculpture in the remains of old Nigerian occupation sites says something about these cultures.

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S. N. NWABARA. *Iboland: A Century of Contact with Britain, 1860-1960*. Atlantic Highlands, N.J.: Humanities Press. 1978. Pp. 251. \$11.00.

It was trade that brought the British into direct contact with the Igbo people of Nigeria during the nineteenth century. After the explorations of the Lander brothers in 1830, Anglo-Igbo trade, the bulk of which was in palm oil, gained prominence. At the early stages the commercial contact was limited to the riverain Igbo kingdoms of Aboh and Onitsha. By the 1860s, when S. N. Nwabara begins his study (although there is no explanation for the arbitrary choice of 1860), commerce was accompanied by Christian missionary evangelism. Henceforth, trade and religion became the dominant forces with which the British were to invade, conquer, and rule Igboland. This is essentially the theme of the book under review, a revised version of a doctoral dissertation for Northwestern University (1965). Its main focus is on the methods of British penetration into Igboland from 1860 to Nigerian independence in 1960.

The author, currently the director of the Institute of African Studies at the University of Nigeria at Nsukka, was one of the early Igbo historians to show serious interest in modern Igbo historical studies. Unfortunately, the publication of his doctoral study was long delayed, presumably because of the outbreak of the Nigerian civil war (1967-70). Consequently, practically all of the basic facts covered in this book have been published in one form or an-

other. Thus, his hope of perhaps being the first to acquaint the reading public with "objective facts about Iboland and the dealings of its teeming population with the people of Britain" has not been realized. In this respect it may be noted that, despite the limited scope of Elizabeth Isichei's *The Ibo and the Europeans* (1973), both studies deal substantially with the same subject. But, because of the limited number of serious historical works on the Igbo, this book may be taken as a valuable addition to the growing body of historical literature on Igbo colonial history. Its textbook format and thematic approach may render it useful to undergraduate students.

For the purposes of this review, it may be convenient to divide the book into three major sections: (1) British penetration of Igboland through trade, religion (Christianity), and education; (2) Anglo-Igbo military encounter; and (3) colonial administration, conflict, and decolonization. The first and the third sections are treated rather cursorily and offer little that is really new. The treatment of Christianity and British commercial relations with the Igbo is rather too spotty and superficial. Although I agree with the author that it would be humanly impossible "to cover every aspect of the interactional process" between the British and the Igbo during the hundred years covered in the study, it is nevertheless a serious omission not to discuss, for example, the nature of Anglo-Igbo trade, especially palm oil trade, and its impact on the society as a whole. The failure, moreover, to examine the relations between the Royal Niger Company and the riverain Igbo states is inexcusable in view of the social, economic, and political consequences of the interaction. The rehearsal of the story of King Jaja's conflict with the British is no substitute for an examination of British relations with Aboh, Onitsha, Obosi, and so on.

From the amount of space given to it (sixty-four pages), there is no doubt that Nwabara considers the military confrontation between the British and the Igbo from 1901 to the end of the First World War as historically significant. And it is. The treatment of patterns of Igbo military resistance to British occupation fills an important gap in the existing literature on West African military resistance to European colonization. The author correctly suggests that, despite their gallant and courageous opposition, the Igbo could not stave off British conquest because of their political fragmentation, sabotage, and lack of coordination and unified command (p. 159). But Nwabara is grossly in error in assuming that the "pacification" of Western Igbo societies or the destruction of the Igwe oracle at Umunoha was based on humanitarian grounds (p. 127). His perception of the Ekumeku movement reflects an un-

critical acceptance of his sources and a failure to appreciate the nationalistic character of the movement. Had Nwabara consulted recent studies on the Ekumeku, and there is no excuse for his failure to do so, he would have perhaps recognized that, contrary to British claims, the Ekumeku society was fighting a "national" cause.

There are a few factual errors. Bishop Crowther died in 1891 not 1892; Archdeacon Dennis visited Owerri in 1905 not 1903; Bishop Johnson not Bishop John was in charge of the Niger Delta Protectorate; Holy Ghost College, Owerri, was founded in 1949 not 1948. Some of the names of towns on pages 119–22 are incorrectly spelled. Finally, as a promoter of Igbo studies, one wonders why the author adopted Ibo instead of Igbo. And why is there no index?

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HOLLIS R. LYNCH, editor. *Selected Letters of Edward Wilmot Blyden*. Foreword by LÉOPOLD SÉDAR SENGHOR. (African Diaspora.) Millwood, N.Y.: KTO Press. 1978. Pp. xxii, 530. \$18.00.

The previous works of Hollis R. Lynch have made the life and published writings of Edward Blyden familiar to students of African history. In this edition of 180 letters written between 1851 and 1910, Lynch permits his readers to penetrate into the mind that produced nineteenth-century West Africa's most exciting historical and social commentaries. There can be no doubt that Blyden deserves this attention, for he expressed the inner life and contradictions of his period better than any of his contemporaries. He offers a private insight into the Africa that could have been. Although no "new Blyden" emerges, the brilliance and compulsions of the man are brought vividly to life. Blyden was an avid correspondent, who sought refuge from a shallow and frustrating domestic situation in communing with more sophisticated minds abroad. Many of his letters were clearly intended to influence those capable of interference in West African affairs; others were meditations in which his struggling soul and creative imagination were given outlet.

Liberia's mission to regenerate Africa, while offering fulfillment to New World blacks, forms a prominent theme in Blyden's letters. The "African Personality," defined in his publications, is given new meaning and ramifications. If, as Blyden taught, the African had a unique and essential role to play in world civilization—a spiritual, communal, selfless contribution—then there were certain implications for participants in the process. More than in his published works, private correspondence al-

lowed Blyden to elaborate his bitter opposition to mulattoes, the racial "mongrels" who lacked a pure African Personality and whose presence retarded African development. In support of racial purity he wrote appreciatively to southern whites in the U.S., approving their segregationist policies as a defense against contamination of the genuine African character, and to American blacks discouraging political action as unsuited to the spiritual mission of the black man. He encouraged Britain's imperial expansion hoping that, as long as it was temporary, it would facilitate opportunities for unifying and educating Africans and for attracting black migrants from the Americas. Education should not betray the innate values of African society, but enable them to reach fruition so that the entire world could benefit from their influence. Though he personally remained a Christian, he condemned the Europeanizing activities of Christian missionaries and preferred the Muslims who built upon an intact indigenous culture. Only a united, confident, and self-conscious black race could realize its divine destiny.

In his foreword Léopold Senghor admits ignorance of Blyden when Negritude was being launched in the 1930s, bearing discouraging witness to the impact of Blyden's teachings. But to modern students this collection validates the African roots of the Negritude and Pan-African movements. The African Personality as proclaimed in the twentieth century was not merely a product of imperialism, for Blyden shows that it was present, and articulated, before imperialism was established. And, because he was a participant and an intelligent observer of African affairs during the European scramble, Blyden's letters contain insights into the period unavailable elsewhere. His correspondents included Gladstone, Brougham, Venn, Lugard, Grmeké, Booker T. Washington, colonial governors, and British and American clerics, academics, and journalists. Published collections of letters and documents are winning increasing acceptance among history teachers. The Blyden letters are a welcome addition to the available resources on West Africa and imperialism, and one looks forward to future volumes in the African Diaspora series from this publisher.

JAMES W. ST. G. WALKER  
University of Waterloo

HILDA KUPER. *Sobhuza II, Ngwenyama and King of Swaziland: The Story of an Hereditary Ruler and His Country*. New York: Africana Publishing. 1978. Pp. xi, 363. \$30.00.

*Sobhuza II, Ngwenyama and King of Swaziland* is a very difficult book to review. On the one hand, the au-

thor, Hilda Kuper, is a long-standing authority on the Swazi nation and has done extensive field work there on this and previous projects. The work, therefore, is rich in detail and depth, and the 347 pages of text contain minutely detailed information about the king, his actions, and the royal house. Particularly useful are Kuper's in-depth descriptions of the monarchy in the 1920s, 1930s, and 1940s as Sobhuza II struggled to maintain the Swazi nation in the face of British colonialism and South African encroachment.

On the other hand, the author's closeness to her subject militates against any truly objective assessment of the monarchy. Indeed, at times some of the author's personal sketches are almost embarrassing, such as when we are told that Bronislaw Malinowski was stepped on by his horse as he picked wildflowers for her. And, being an "official" biography, Sobhuza and all his actions are presented in such a favorable light that the document eventually loses a great deal of utility for other scholars, particularly when the modern period is covered. For example, when the king, irritated by parliamentary opposition, scrapped the constitution in 1976, we are told this was merely "an effort to turn nominal political independence into full sovereignty under a leader who had proved his wisdom and moral courage over the years" (p. 337). What begins as a historically useful effort eventually turns into a virtual paean, so cosmetic as to obviate much of its not inconsiderable value.

CHRISTIAN P. POTHOLM  
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JOHN DUGARD, *Human Rights and the South African Legal Order*. Princeton: Princeton University Press. 1978. Pp. xix, 470. Cloth \$27.50, paper \$12.50.

In South Africa, apartheid is the rule of law. Parliament alone is sovereign. Its laws—any laws—are apparently supreme in the absence of a system of judicial review. The legal profession has seemed powerless or unwilling to protect human rights against discrimination and repression. This is the situation John Dugard authoritatively describes, examining the South African constitution, statute law, political trials, and judicial behavior. His approach is historical and comparative, referring to the United States and Great Britain in documenting South Africa's legal evolution away from the norms and values of Western civilization. His stated purpose is not to judge but to describe and explain, yet to achieve the latter seems to necessitate the former. He hopes and prescribes for reform.

Dugard argues that, contrary to popular myth, common law rights no longer remain the unequivo-

cal basis of South African law and justice, having been eroded in legalizing apartheid and suppressing dissent. Security legislation and political trials exclude the writ of habeas corpus, deny judicial oversight of police, allow unrestrained interrogation, withhold speedy trial, permit double jeopardy and splitting of charges, presume guilt, and provide for self-incrimination. Thus, the South African legal order, its increasingly repressive content and spirit essentially unchallenged by the legal profession and judiciary, is a pressure cooker set to explode under its own society. Dugard puts the onus upon the tradition of parliamentary supremacy coupled with an uncritical, even slavish, acceptance of legal positivism. Its extreme expression he finds in an appellate court decision as early as 1934: "Parliament may make any encroachment it chooses upon the life, liberty, or property of any individual, and . . . it is the function of courts of law to enforce its will" (pp. 35–36). This doctrine has disarmed the legal profession into losing the distinction between the rule of law, conceived as due-process safeguarding libertarian values and substantive freedoms, and rule *by* law, seen as a set of legal rules for maintaining executive power.

Dugard believes South Africa's legal profession can restore "the prime value of the South African legal tradition—the worth of the individual" (p. 383) by turning to natural law theory and a jurisprudence informed by a legal realism that recognizes that as it makes judgments the judiciary is inevitably the shaper, not merely the enforcer, of the law. Ignoring this reality, it falls prey to "unarticulated premises," tending increasingly to serve popular prejudice and to favor the status quo. Asserting their interpretive powers and accepting the reality of these premises, "it will be easier for judges to be guided by accepted legal values rather than by subconscious prejudices" (p. 382). Here this reviewer balks. Surely the "unarticulated premises" to which he refers are the very articulate premises of the political order: white supremacy. Parliament in its statutes says so. Who is likely to prevail in the confrontation between judiciary and state that his solution implies? Yet Dugard sees no other alternative: restore the primacy of Western legal values *cum* human rights before it is too late, or ultimately the leaders of a new black majority, reared on the apartheid legal order, may find these values and rights inexpedient for their own domination.

This is an important and scholarly though somewhat repetitive book. It appears at a critical time. Indeed, the condition it diagnoses appears so dangerously advanced that the prescription may be merely palliative. One finishes reading still unsure that the conception of human rights equated with Western individualism has power to contend

with the spirit of authoritarian collectivism that animates South Africa's tragic conflict.

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## ASIA AND THE EAST

JOHN K. FAIRBANK, editor. *The Cambridge History of China*. Volume 10, *Late Ch'ing, 1800-1911*, Part 1. New York: Cambridge University Press. 1978. Pp. xvi, 713. \$45.00.

At the inaugural reception for this first volume of the landmark, fourteen-volume *Cambridge History of China*, a distinguished panel ruminated anew on the problem of presenting that history in English. Would not the Chinese viewpoint be submerged?

The editor, in his powerful and sympathetic review of the old society, surveys other perennial questions: among others, the riddle of population growth; the aptness of the dynastic-decline notion; the relative importance of internal and external elements—"feudalism" versus "imperialism"—in that decline. Most of the contributors cope with these three posers.

Joseph Fletcher makes three contributions on China's vast inner frontier arc. The surging Han Chinese population trampled the dynasty's Central Asian ethnic pale and modernized many of the areas it reached. Indeed, he asserts, China would have modernized itself—although this unusual refutation of China's "decline" tends to disappear in tracts of socioethnic description and thickets of romanization. Ch'ing application of inner barbarian experience to the maritime West is clearly shown in the prior relationship of the unequal and forgotten 1835 Kokand treaty to the 1842 Nanking treaty. Here priority is assigned to "internal" over "external" systems. Similarly, it was less Russian imperialism that cost so much territory in 1858-60 than it was the Ch'ing denial of Manchuria to land-hungry Chinese. The assignment of this much space to the Asian arc is an implicit editorial contribution to the debate.

Susan Mann Jones and Philip A. Kuhn use the same population increase to explain a ruinous clogging of domestic institutions. The 1796-1805 White Lotus rebellion was more telling on the dynasty than the later imperialist Opium War, but the real turning point came in the growing commercialism and privatism of the 1780s, which so concerned the thinkers of the early-nineteenth-century "statecraft school." This pre-imperialist deterioration, say Jones and Kuhn, was more advanced than contemporaries knew, a judgment that returns us to the "Chinese outlook" difficulty. On the other hand, we

are not studying a neat *yang-yin* paradigm of decline. Jones and Kuhn, as well as Fletcher, see dynastic vigor even late in the century, not least in its survival of the Taiping Rebellion.

The intricate, flourishing, and unstable Canton trade is closely analyzed by Frederick Wakeman, Jr., who discloses that native and foreign monopolists there were in closer league than their governments cared to countenance. He does not put the same stress on cultural confrontation that the editor does, which may explain why certain passages in his coverage of the Opium War savor of opera bouffe. The antecedent inflation was due less to the opium drain of silver than to Ch'ing debasement of copper—another accent on the internal component.

The editor himself discusses the Nanking treaties. The British and the Manchus were after all both conquerors and made a synarchic application of the new system. Indubitably it was imperialistic, but there was an underlying internal rift, as Fairbank suggests in a closing reference to the "default of the conservative mandarin and scholar gentry . . ." (p. 265).

Philip Kuhn returns with an analysis of the Taiping Rebellion. He wonders again if "decline" was holistic; the rebellion itself—he calls it "a profound social revolution" (p. 279)—was "an authentic response to the distinctive problems of the late imperial age" (p. 317). Although he stresses the indigenous nature of the movement and its program, it was alien Christianity that energized the whole. Kuhn carefully compares the Taiping to the Nien Rebellion, which, being more traditional and country-based, was ultimately defeated but not, as was the Taiping movement, *obliterated*.

Kwang-ching Liu holds that the T'ung-chih Restoration enabled the regime somehow to survive and so joins his colleagues in questioning the received wisdom on "decline." For him, that "restoration" ended in 1868, being vitalized mainly by the convulsions of rebellion. Even so, Tseng Kuo-fan was often balked by the corruptions of the men on whose supposed Confucian excellence he placed such reliance. Liu recounts, in perhaps excessive detail, Tseng's efforts to eliminate abusive surcharges in "liberated" areas. Whatever he might have hoped, his reforms favored the gentry, and, when the tumult died, the old regime reasserted itself.

The late Ting-ye Kuo and Liu give a view of the "self-strengthening movement." Here was Confucian reform, transforming the "statecraft" search for good men into one for military strength. No institutional space was cleared for the goods or men involved, although by the late 1870s the movement shifted from defense alone to industrialization. One wonders if there is anything distinctively Chinese about the Kuo-Liu perspective.



Might it be that the intellectual side of the effort gets three times the space assigned to the nuts and bolts? Or is it that in the associated bibliographical essay a "Confucian" attention is given to literature, pro and con, on the purity of Li Hung-chang's patriotism? Perhaps, but the questions are really universal.

The volume closes with Paul A. Cohen's contribution on the missionary movement to 1900. The anti-Christianity of the conservative gentry was well grounded, although partly targeted on the treaty-encumbered dynasty. Missionaries were inadvertent revolutionaries, not in their gospel but in their secular offerings to their students. Missionary reformers were at apogee in the abortive reforms of 1898, but, after the Boxers, critics of missions were themselves Chinese revolutionaries seeking autonomous secular change.

The scholarly apparatus of this volume is commensurate with its other contents. Bibliographical essays often include brief review articles; for example, Liu, who believes that the dynasty did not dissolve into a regional cluster, identifies recent scholarship that recycles the question. The fifty-page bibliography lists works in many languages, including two hundred and fifty East Asian titles, mostly from Taiwanese scholars or publishers but with sixty mainland imprints and nearly that number from Japan.

The editor's presence in this volume is commanding, as it is in the larger field. All invited contributors save Kuo were students of his; most incline to his "how-did-it-work" institutional approach; five of the eleven chapters are shared by two Harvard contributors; and university press citations show an overwhelming preponderance of that university's logo. If there is a theme to this solid and complex volume, it might be that China failed to meet its potent nineteenth-century challengers mainly for internal reasons—but its decline was more complex than we know.

JOHN L. RAWLINSON  
Hofstra University

PATRICIA BUCKLEY EBREY. *The Aristocratic Families of Early Imperial China: A Case Study of the Po-ling Ts'ui Family*. (Cambridge Studies in Chinese History, Literature and Institutions.) New York: Cambridge University Press. 1978. Pp. vi, 240. \$22.95.

This study of a prominent family from the first through ninth centuries stands beside David Johnson's recent *Medieval Chinese Oligarchy* (1971) as a sign of new life in American scholarship on early Chinese history, a field that is notoriously difficult because of the paucity of primary sources. Patricia

Buckley Ebrey uses excavated funerary inscriptions from the sixth to tenth centuries to flesh out the information in dynastic histories and miscellaneous writings in order to delineate the fortunes of a great family and through this case study to approach broader social and political issues concerning the relationship of such families to the Chinese state and society. In doing so, she has unearthed fascinating themes of great interest to social and political historians of medieval societies elsewhere around the world as well as to Chinese historians of subsequent periods.

The Ts'ui family originated in northeast China, in the modern province of Hopei: Po-ling was the prefectural name attached to their home place and adopted by them as an identifying adjunct to their surname. In the Han (202 B.C.-A.D. 9, A.D. 25-220) dynasty, the Ts'uis were local magnates who rose from provincial prominence to participation in the upper-class society of the capital. Their strong adherence to their home base enabled them to survive political vicissitudes and provided autonomy from imperial politics. Yet they already followed Confucian norms, not only in their personal lives but also in their movement to government service.

These characteristics became more marked in subsequent periods, as the Ts'uis severed their links with Po-ling, moved to the capital, and joined in the competition for office. Unlike their counterparts in Europe and Japan, Chinese aristocratic families did not become feudal lords but bureaucrats.

How does a family perpetuate its elite position for almost a thousand years? Not by the strength of its family organization, according to this book. During the T'ang the Ts'uis were a scattered group of high status families who were not a lineage, if by that we mean an organized kinship group with common activities. Available materials do not permit Ebrey to explore the economic foundations of the family and how such family atomization affected downward mobility, given China's system of partible inheritance. Her sources provide information on only the most famous Ts'uis' successful strategies of upward mobility. We learn much about marriage relations: the Ts'uis consistently confined themselves to intermarriage with other prominent families, the most successful marrying only those belonging to the "seven families" of high pedigree identified in the seventh century. T'ang recruitment into the bureaucracy favored those with connections; the regulations permitting sons and grandsons of highest officials special entry into academies or minor posts explains why there is a three-generation span in the eight notable Ts'ui lines of the period. But most Ts'uis occupied relatively minor offices, which supports the contention that merit, not birth, was increasingly the criterion for



promotion. The Ts'uis, who combined awareness of their historical heritage with acceptance of the shift from ascriptive to achievement standards, symbolize the transition from an aristocratic to a bureaucratic age in China.

EVELYN S. RAWSKI  
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S. Y. TENG. *The Taiping Rebellion and the Western Powers: A Comprehensive Survey*. 2d. ed. Taipei: Yee Wen Publishing. 1977. Pp. xii, 458. \$8.00.

As a popular movement that had perhaps over a million followers at its peak and that caused the loss of some twenty to forty million lives as well as the devastation of many of China's richest provinces, the Taiping Rebellion was unparalleled until the present century. An even more significant claim to uniqueness was the revolutionary character of the movement itself: a system of equal distribution of wealth, communal ownership, mass mobilization of its followers, all buttressed by a Protestant-derived ideology. Why did such a movement emerge in mid-nineteenth-century China? What was its real character? Why did it fail? And what was its impact on subsequent history? More than two thousand studies have attempted to provide an understanding of the problem. Does the volume at hand have something to offer?

Those familiar with the history of the Taiping movement will have little to learn from S. Y. Teng's analysis of its causes. For example, to what extent can we say that "the Taiping Rebellion was a direct outgrowth of the Opium War"? It is undeniable that the war exacerbated the degree of social disintegration to which the Taipings were eventually compelled to respond by force. But that does not explain why the Taipings espoused a Western, Christian doctrine or why the Taipings, except for their anti-Manchuism, were not particularly anti-foreign. Did the movement represent a non-Mandarin's response to the West? Was it a search for cultural-spiritual parity? These are questions worth investigating.

A major theme of the work deals with Taiping-Western relations. Here I am inclined to agree with Teng that Western, particularly British, "neutrality" was very much dictated by commercial interests that favored the Ch'ing. Subsequent armed intervention in Shanghai calls for no surprise.

The last sections of the book go into the more-or-less familiar reasons for the rebellion's failure, its impact on other rebellions (with unduly long descriptions of these movements), and its influence on late Ch'ing government and society. By limiting his analysis to the Ch'ing period, Teng omits the very

important heritage of the Taiping Rebellion in our own century. Few subjects in Chinese history have been studied by so many. Despite its failure, it called into question the foundations of Chinese society. In an implicit way, it raised the problems of how a society could be reordered. Thus, for many, it remains a heroic attempt at correcting China's sociopolitical ills.

Occasional insights and interesting details notwithstanding, the work at hand suffers from trying to be too comprehensive. Thus, after thirty-odd pages of Taiping relations with Western missionaries, we are told that "the actual influence of missionaries was either unimportant or negative." On the other hand, missionary reports were not fully exploited as a valuable source of historical data. The inclination to examine parallel developments separately rather than chronologically also detracts from the flow of the narrative.

Finally, the attempt to present a balanced account often leads to the citing of opposing views with minimal judgment on the part of the author. To suggest that, "since the basic structure of society was not altered, and the Taipings did not make any 'permanent change of a good kind' or 'progress,' it should still be called the Taiping Rebellion" certainly does not provide an adequate basis for an understanding of what constitutes a revolution. Teng's "neutrality" is fraught with inherent predispositions, as British "neutrality" was a century ago.

DAVID PONG  
Australian National University

GERALD S. GRAHAM. *The China Station: War and Diplomacy, 1830-1860*. Oxford: Clarendon Press. 1978. Pp. xvi, 444. \$39.00.

This well-crafted narrative adds another strand to the now finely knit story of Britain's opening of China. It was, after all, done mainly by the Royal Navy in the uncharted waters of the China coast, and Gerald S. Graham gives us graphic and judicious accounts of the major naval actions in south, central, and north China.

Begun as a byproduct of his major work, *Great Britain in the Indian Ocean, 1810-1850* (1967), the present volume likewise makes good use of the admiralty records in the PRO and traces the interaction among naval and civilian authorities in London, Calcutta, Trincomalee, Hong Kong, Shanghai, and way stations. The result is a sort of intra-imperial diplomatic history that shows how the different arms of British expansion worked out their respective functions from stage to stage. The China coast merchants are represented by apt quotations from the Jardine Matheson files at Cam-

bridge. Private papers in England were consulted as well as archives in New Delhi and elsewhere, but the now extensive corpus of books and articles by modern historians of Sino-foreign relations is used only superficially.

This is probably because the author modestly set out to abjure pontificating on Chinese ways and on the inscrutabilities of Chinese policy and tried to concentrate instead on the problems and achievements of the Royal Navy. Yet the opening of China is such a rousing story of Victorian adventure in ancient Cathay that he was seduced inevitably into taking a fresh look at the Bluebook history first summarized by H. B. Morse in volume one of the *International Relations of the Chinese Empire*—especially for the 1830s at Canton. The resulting study makes extensive use of contemporary records like the *Chinese Repository* and numerous memoirs but often neglects later scholarship. He treats the incident of the lorcha *Arrow*, for example, as Morse did in 1910 and Costin in 1938 without noting the revisionist articles of J. Y. Wong that cast doubt on Harry Parkes's veracity about the lorcha's flying the British flag. Commissioner Lin Tse-hsü (not Tse-haü Lin [p. 85]) and other Chinese remain as two-dimensional as they were in the *Repository*. Thus, on Sino-British relations in general (not his main interest), Graham jumps only part way across the river, while on the British navy in China he makes many astute and cogent contributions. His summaries of policy considerations and his appraisals of leading British personalities are neat and illuminating. The volume is a sophisticated addition to the literature.

JOHN K. FAIRBANK  
Harvard University

SUZANNE PEPPER. *Civil War in China: The Political Struggle, 1945–1949*. Berkeley and Los Angeles: University of California Press. 1978. Pp. xxi, 472. \$18.50.

The catastrophic collapse of the Kuomintang regime in China during 1945–49 and the Communist victory on the mainland have left a whirlpool of controversy. Did the Kuomintang fall, or was it pushed? In Suzanne Pepper's words, "did the Chinese Communists win a genuine mandate to rule, or were they primarily the beneficiaries of KMT mistakes and Japanese excesses?" (p. 6).

Communist success in mobilizing the peasants during the war against Japan has been the subject of several studies, and we have some excellent eyewitness accounts of the civil war period; but until now we have lacked a comprehensive, documented examination of the competition between the two parties in the cities as well as in the countryside.

On the whole, this book achieves its purpose.

Pepper draws on a variety of sources, including Communist and Kuomintang documents and the writings of Chinese intellectuals, to show in fascinating detail how the Kuomintang, through incompetence, venality, and economic mismanagement, succeeded in alienating almost every section of the urban population.

The KMT's insistence on waging civil war against the Communists, according to the author, cost it the support of the intellectuals, who were convinced that neither side could win militarily and who concluded that the price of the war was too high in terms of the postponement of needed reform. Harried by inflation and repelled by the government's incompetence, "each group, in its own way and for its own reasons, withheld support for and cooperation with the Government in its war against the Communists" (p. 427). Nevertheless, the urban middle classes "did not actually abandon the KMT until its fate was sealed on the battlefield" (p. 129).

Pepper describes the impotence of the liberals, who dominated the intellectual life of China but had no program of their own. "They were," she observes, "too far removed from the forces that defined the nature and the meaning of the struggle and would therefore determine its outcome. Perhaps their greatest tragedy was [that] they failed to comprehend the fact of their own irrelevance" (p. 194).

The second half of the book analyzes the successes of the CCP, which the author attributes to the "flexibility and patience with which it adapted its struggle for power to [its] environment, step by step and directive by directive" (p. 433). Taking a fresh look at land reform during this period, Pepper contends that in North China, where most peasants owned their own land, "tenancy itself was not a problem that concerned the village masses" (p. 237). But where landlordism was not a burning question, she says, the party adjusted its policy to encompass a wider range of issues that provided a formula for mass activation through class struggle. Such flexibility, the author claims, made it possible for the Communists to gain a wide base of urban support once the KMT had collapsed. Thus, in order to maximize production, the party courted the bourgeoisie and restrained the militant workers, choosing to "safeguard at least temporarily the position of the capitalist exploiter against the demands of a relatively advanced labor movement" (p. 377).

Although some of these interpretations may be challenged, few will dispute the author's overall conclusion that "politically, the CCP's victory was just as valid as the KMT's defeat" (p. 433). Pepper does not come to grips with the questions of whether the KMT was capable of transcending its

inner rot and reforming itself, or whether peace with the Communists and a coalition government was ever a realistic alternative. Nor does she fully examine the implications of the CCP's policy of curbing the labor movement and its relevance to the struggles of the 1960s. But her book deserves to be read by every serious scholar in the field.

WALTER E. GOURLAY  
Michigan State University

TETSUO NAJITA and IRWIN SCHEINER, editors. *Japanese Thought in the Tokugawa Period, 1600-1868: Methods and Metaphors*. Chicago: University of Chicago Press. 1978. Pp. xiii, 209. \$18.50.

Tokugawa thought was exciting, full of change and foreshadowings of the transformation of "modern" Japan. That is the message this book presents. It comes in eight new essays, four by American specialists and the other four by Japanese scholars. The book thus sets out to locate in the indigenous intellectual tradition those parallels with institutional change that signify the ultimate importance of the Tokugawa period as the seedbed of Japan's modernization. But in this volume the editors have essentially overlapped the modernization controversy by discussing it only in their introduction.

Tetsuo Najita, Irwin Scheiner, and Harry Harootunian open the book by challenging the stereotype of a tiresome Tokugawa tradition in thought. Not development so much as discontinuity and rupture fill the pages of their essays. Each introduces at least one metaphor that may help us understand why change was occurring. Najita writes of "fusion" in the thought of Kaihō Seiryō, a pragmatic inclination (away from "epistemological convergence" earlier in the eighteenth century) urging the commercial stratum to play a more active economic role. Scheiner uses "covenant" as an analogy for the "social promise" between *daimyō* and villagers, reminding us what a broken covenant implies. Harootunian sees "archaicism" as a strategy employed in the texts of late Tokugawa nativists (*kokugakusha*) for attacking rigidified world views and prefiguring new political departures.

Harootunian's work deserves special attention. Here he resumes his assault on conventional intellectual history with its emphasis on rationality and the continuity of ideas. As he strives to loosen these fetters, he produces much that seems opaque and will frustrate readers. But the trouble is worth it. He situates in the rhetoric of the nativists that mythic transformation that he insists was necessary to arrest standard political discourse and so prepare the way for a radically altered future.

The other essays also deal with challenges. Bitō

Masahide, Matsumoto Svannosuke, and Robert Bellah all question the significance of Ogyū Sorai as a harbinger of modernity in Japanese thought, thereby challenging Maruyama Masao's famous thesis. Bellah also reconsiders his own *Tokugawa Religion* (1957) and its approach to the prefiguration of change that occurs in Tokugawa thought. Sakai Yukichi shows that Tokugawa Neo-Confucianism determined the shape and content of Inoue Kowashi's constitutional theorizing—an area where we would expect Western influence to be decisive. And Niiyama Shigeki offers a Saussurian exploration of the Japanese language as a medium for conveying ideas about reality. Niiyama suggests that any language is a "cultural code" that indicates how the culture goes about classifying the perceived world. We must wonder whether Tokugawa thinkers were asking more than our own classificatory systems allow us to see.

Nothing in any language deals with Japanese intellectual history as this book does. In it we find murmurings of transdisciplinary inquiries pioneered by the structuralists, by Michel Foucault, and by Hayden White in his baroque masterpiece *Metahistory* (1973). Shards from the wars over linguistics appear in several essays, and the impact of literary criticism is heavy.

The editors have blended eight essays into a valid unit. They and their fellow authors signal the advent of a new Western attitude toward Japanese thinkers of the eighteenth and nineteenth centuries. If the book fails to demonstrate (but only asserts) that this was a highly creative era in comparative intellectual history, the assertion encourages those of us who believe it anyway, and it dares others who demur to re-evaluate their premises. In the end it challenges us all to restudy our perceptions of Tokugawa thought—an achievement that makes this an important and compelling book.

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JOSEPH E. SCHWARTZBERG, editor. *A Historical Atlas of South Asia*. (Association for Asian Studies, Reference Series, number 2.) Chicago: University of Chicago Press. 1978. Pp. xxxix, 352, 5 inserts. \$150.00.

*A Historical Atlas of South Asia* is an extraordinary work of scholarship in several respects. Six hundred and fifty of the seven hundred maps have been specially prepared for this book and many represent the results of new analyses of primary data. Both the maps and the text, which alone is the equivalent of over four hundred ordinary-sized pages, provide fresh historical interpretations by some of the world's leading South Asian scholars.

The atlas project, begun in 1964, was conceived by Jan Otto Marius Broek and Burton Stein and executed under the direction of Joseph E. Schwartzberg of the University of Minnesota. The research cost roughly \$1,000,000 and the publication expenses were over \$200,000. Donations have enabled the publishers to sell the book at less than half the manufacturing cost.

The maps and texts are separate, although most maps contain extensive explanatory notes. The maps not only clarify spatial relationships between historical phenomena but also attempt to reveal sequential development and patterns by noting on a single map major events and changes over a period of decades or centuries. Thus, these maps do what maps in most historical works fail to do: they suggest the dynamic character of evolution rather than freeze development at a particular point in time.

Many of the maps on early periods delineate the geographical extent of prehistoric cultures and post-Harappan dynasties. Past disagreements about the limits of major empires are illustrated: there are ten views of the frontiers of the Kushana Empire and nine views of the Mughal Empire in 1605.

Pre-Muslim India lacked traditions of historical writing comparable to those found in China or the West. The authors of *A Historical Atlas* have provided examples of the evidence used to reconstruct South Asian history by including on the map plates photographs and drawings of archeological sites, artifacts, sculpture, monuments, portraits, coins, and inscriptions as well as quotations from contemporary texts, examples of calligraphy, chronological and dynastic charts, and administrative diagrams. The authors have also included maps and diagrams illustrating how South Asians viewed their contemporary world. Thus, a plate on Puranic India (Bharata) contains four illustrations: a map of natural features and sacred places mentioned in the Puranas; a map of Puranic regions, peoples, and cities; a diagram of the Puranic conception of the world, with Mt. Meru at its center; and a photographic reproduction of a fragment of a Jain manuscript depicting a Jain conception of man's universe. Such material helps to reconstruct contemporary, subjective views of the design of the cosmos, regional differentiation and integration, and knowledge of physical, political, and sacred geography.

With the Muslim, British, and post-Independence periods, the data available to be mapped increases dramatically. The maps and text on "Imperial India and the Growth of National Identity," "Post-Independence Political History," "Modern Social and Cultural Evolution," and "Modern Demographic and Economic Evolution" are diverse and rich. They contain cartographic and textual analysis of the more obvious categories of popu-

lation, literacy, religious communities, migration, railway, road, and canal construction, factory employment, and election results that are derived from the decennial censuses and government reports. They also enable the reader to see the broad South Asian patterns as well as regional variations that are so often obscured in both national and regional studies. For example, a reader interested in the character of rural society in a particular region or district will find maps and discussion of village house types, settlement patterns, land tenure systems, and major crops in addition to the most numerous peasant, landholder, mercantile, and depressed castes. A person seeking a fuller understanding of South Asian languages will find a taxonomic map illustrating the relationships between language families, major languages, and dialects. There are also maps showing the proportional use of scripts and of predominant, minority, and subsidiary languages. The text on language, as on other subjects, explains the significance of what is mapped and discusses the problem of evidence and interpretation. For example, it analyzes the difficulty of using the censuses of India and Pakistan to identify mother tongues and observes that there is little indication that Hindi, the national language of India, is spreading among the 55.5 percent of Indians whose mother tongue is not Hindi or a mutually comprehensible language.

The atlas contains a bibliography with four thousand items organized into ten principal categories, an index with sixteen thousand entries, two overlay maps (Physiography and Administrative Divisions, 1975), and three detached chronological charts. Altogether, the book is a superb triumph of scholarly achievement, collaboration, and organization. It deserves to be in any reference collection and in the private library of all teachers concerned with the seven countries of South Asia and their neighbors in the rest of Asia.

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CLIFFORD EDMUND BOSWORTH. *The Later Ghaznavids: Splendour and Decay. The Dynasty in Afghanistan and Northern India, 1040-1186.* (Persian Studies Series, number 7.) New York: Columbia University Press. 1977. Pp. vi, 196. \$15.00.

The story of the later Ghaznavids has been neglected by historians, perhaps because the sources for the late eleventh and twelfth centuries are far less satisfactory than those for the period of Sebuktigin (977-97), Mahmud (998-1030), and Masud I (1030-41). Clifford Edmund Bosworth has now provided a worthy successor to his *Ghaznavids, Their*



*Empire in Afghanistan and Eastern Iran, 994-1040* (1963), confirming his stature as the outstanding contemporary historian of the eastern Islamic world prior to the Mongol conquests, and in that respect the heir of Barthold and Minorsky.

The nemesis of early Ghaznavid expansionism appeared in the form of the Seljuks, whose occupation of Khurasan was sealed by the overwhelming victory of Dandaqan (1040), after which the westernmost limits of Ghaznavid rule followed a line running roughly southwest from Parwan in the Hindu Kush through Ghur to the lower Helmand. As a result of this exclusion from the Iranian plateau, the Panjab acquired an increased importance for the Ghaznavids, with Lahore becoming, in effect, a second capital.

With the death of Masud I in 1041 there began a phase of almost unrelieved instability, five sultans reigning as colorless mediocrities or worse between that year and 1059. It was during this "Time of Troubles," as Bosworth describes it, however, that two important trends began to emerge: the establishment of a *modus vivendi* with the Seljuks and renewed expansion beyond the Sutlej. The stage was thus set for the long and impressive reign of Ibrahim (1059-99), which was characterized by the maintenance of relatively harmonious relations with the Seljuks, Alp Arslan and Malik Shah; an attempt to strengthen the largely nominal Ghaznavid hegemony over Ghur (Bosworth wisely cautions against Juzjani's assumption of a Shansabanid domination of Ghur as early as this reign [p. 69]); and the capture of Agra, with its disturbing effects upon the political status quo in the Jumna-Ganges Duab.

Masud III (1099-1115) continued the work of consolidation and realignment begun by his father, and the momentum was further sustained, at least temporarily, by his successor, Bahram Shah (1117-1157?). Bahram Shah only obtained the throne through the intervention of the Seljuk Sanjar, however, and hence the last Ghaznavid rulers were reduced to the status of Seljuk vassals. Bahram Shah's reign saw a flowering of court culture comparable to that under the first Ghaznavids, but the portents that the regime was near its end were unmistakable: Sanjar's devastation of Ghazna in 1117 in the course of seating Bahram Shah on the throne, and again in 1136, when the latter temporarily asserted his independence; its capture in 1148 by Saif al-Din Suri of Ghur; and the celebrated sack of 1150-51 by Ala al-Din Husain, his brother. The last two Ghaznavids, Khusrau Shah (1157?-60) and Khusrau Malik (1160-86), were forced to abandon Ghazna for the relative security of Lahore, where the latter survived until 1186, when he fell into the hands of Muizz al-Din Muhammad of Ghur.

This complicated story of imperial and dynastic

decline has been carefully reconstructed by Bosworth from often conflicting sources, and he deserves particular credit for the way in which he has made use of contemporary literary references, and especially the diwans of Uthman Mukhtari, Abul-Faraj Runi, Sanai, and Sayyid Hasan. This is a work of the highest scholarship, to be regarded in every sense as definitive.

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PRATIMA BOWES. *The Hindu Religious Tradition: A Philosophical Approach*. Boston: Routledge and Kegan Paul. 1977. Pp. ix, 322. \$18.95.

At the beginning of her study, Pratima Bowes makes a distinction between the social role of a religion that is introduced through the activity of a particular founder and one that crystallizes over a long period of time out of features within the fabric of a society. Christianity is the obvious example of the first variety, an essential feature of which is the opposition by the founder and his disciples to salient aspects of their society. Tension with society is at the heart of such religious systems. Hinduism, by contrast, Bowes argues, has an organic relationship to its social milieu that, denying tension, gives a religious dimension to all social behavior and makes it a part of the sacred traditions.

On the basis of this understanding, Bowes proceeds to give an analysis of the Hindu tradition that should be of interest to nonspecialists who want a competent guide to possible interactions of religious beliefs and society in India. Specialists will also find much that is stimulating in her well-argued generalizations, which are based on wide reading and sympathetic understanding of the belief systems of modern Hindus. How aware Bowes is that she is essentially interpreting the past through the prism of the contemporary religious situation is not clear, for she accepts rather uncritically the common belief that the varieties of beliefs and practices in India can be explained by reference to a historical process of the coalescence of numerous traditions, races, and tribes. There is, on the contrary, good historical and anthropological evidence to suggest that the varieties of Hinduism are not so much survivals of the past as relatively recent developments. This does not vitiate, however, her interesting account of what she calls "relevant Hindu beliefs," such as *karma*, transmigration, and theories of time.

Bowes handles with considerable success the problem, difficult in all religious traditions but especially so in Hinduism, of defining the concept of truth. She does so by a comparison of scientific reality and religious reality. Just as there is no absolute



truth for the scientific understanding of nature, so also in the Hindu religious tradition there is no single framework of inquiry. The formulation of inquiry depends, in both religion and science, upon cultural and historical factors that vary in time and place, producing different and even contradictory statements. These are reconciled, she argues, within the Hindu tradition by reference to the concept that truth, though not relative, can be stated in a variety of ways, depending upon experience. In this formulation, it is the experience, not "truth," that needs validation and examination.

Bowes argues that, as a culture form, religion has two characteristic aspects. One has to do with values, such as love, self-transcendence, compassion. The other aspect is institutional and is directly related to social practices. Hinduism—having no church, no original priesthood, and no Book—found in social customs the cohesive force that saved it from disintegration under the weight of internal and external pressures, such as invasion and domination by alien peoples. From this she argues that such social practices as caste can be replaced by alternatives that, while consonant with contemporary political and social beliefs, can be grounded in what she regards as the "relevant values" of Hinduism.

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BARBARA N. RAMUSACK. *The Princes of India in the Twilight of Empire: Dissolution of a Patron-Client System, 1914–1939*. Columbus: Ohio State University Press, for the University of Cincinnati. 1978. Pp. xxii, 322. \$20.00.

Before the advent of independence and partition in South Asia in 1947, princes ruled over two-fifths of the subcontinent. Their states ranged in size from tiny areas that E. M. Forster characterized as befitting a Gilbert and Sullivan operetta to territories as large as many European nations. Yet, as is evident from the secondary literature cited in *The Princes of India*, there is a considerable lacuna in the scholarship on princely states. Not only are there enormous gaps on the internal workings of these states, but also there is surprisingly little on "high politics."

This study seeks to fill the latter hiatus by focusing on "those rulers who became active in all-Indian politics during the period between 1914 and 1939 . . . to discover what options were open to them when the balance of power was changing in an imperial system" (p. xvi). Although she does not depart significantly from the seminal research of Lloyd and Susanne Rudolph on Rajputana, Barbara N. Ramusack extends the analysis of policies

and choices in British relations with princes to the entire group of aspiring "imperial politicians," particularly the princes of Alwar, Baroda, Bhopal, Bikaner, Gwalior, Hyderabad, and Patiala.

From the outset the changing political realities of the early twentieth century placed the princes in a quandary. The growing Indian nationalist movement threatened not only the power of their "patrons," the British, but also their right to rule. And, although support for the Empire was imperative, nevertheless, as "clients" they had to guard against further erosion of their own authority, especially in matters relating to minority administration and succession procedures. British control over the states, moreover, was exercised by "paramountcy," a form of rule that was never clearly defined. Nor were the princes allayed by the Butler Commission of 1928, which responded to their request for the demarcation of the limits of government power by declaring that "paramountcy must remain paramount" (p. 150).

Furthermore, although the rising tides of nationalism necessitated their greater involvement in British Indian affairs, the princes were not able to emerge as effective political figures. Their options were limited because the government was not willing to let them "forge any alliances with British Indian groups that might threaten British power . . . [or] to bring the princes into the decision-making process . . . and to evaluate seriously princely suggestions on political reform in British India" (p. 47). And, since they were not unified, even the forum for their interests, the Chamber of Princes, was riven by in-fighting and served instead as a channel for the government's concerns. Small wonder, then, that the passing of the Empire in South Asia led to the integration of the princely states into the new nations of India and Pakistan.

Although the interactions at the macro-level are well documented and impressively detailed, this book makes no attempt to relate them to structures and processes existing below that level. Consequently, its application of the patron-client concept acts primarily as a description of a relationship rather than as a building block and framework for understanding paramountcy and its effects on the wider society and polity. As the first systematic survey of relations between the British and the princely states in the twentieth century, however, this study is mandatory reading.

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JOHANNES H. VOIGT. *Indien im Zweiten Weltkrieg*. (Studien zur Zeitgeschichte, number 11.) Stuttgart: Deutsche Verlags-Anstalt. 1978. Pp. 413.

This formidable volume is an expanded doctoral dissertation presented to the University of Stuttgart under the title "Indien im Zweiten Weltkrieg, 1939–1942." Naturally, these early years of the war absorb more than half of the completed study. Moreover, in the manner of most dissertations, the narrative is heavily laden with factual material that occasionally obscures the analytical theme.

Yet an overall thesis does emerge. Johannes H. Voigt combines military and political history and thereby claims, with some justice, to have made a pioneering effort, at least as far as wartime India is concerned. In military terms, he makes the geopolitical observation that India lay at a point of intersection between European and Asian theaters of war. He suggests that India might have been the object of Japanese operations in Southeast Asia and of a potential German advance through the Caucasus. For the Anglo-American fleet, India was the vital supply base east of Suez.

On the political side, he sets the development of the Indian independence movement in the context of war emergencies in Asia. This perspective reveals that Gandhi, while preparing the Congress for acceptance of his "Quit India" resolution, was more alert to Tojo's statements before the Japanese Diet than to General Wavell's strategic concerns after the fall of Singapore. When the Anglophile Nehru protested that Gandhi's scheme of passive resistance against Japanese aggression was defeatist, he was ignored. Meanwhile, Subhas Chandra Bose assumed the aspect of a hero who "risked much for India" (p. 165). The author also, parenthetically, brings into prominence the role of the United States. Both Gandhi and Nehru nurtured suspicion of American emissaries taking an interest in India at the very moment that British power appeared to be in decline. In Nehru's words, there existed a "fear that America, with all her vast resources, may either dominate India herself, economically if not politically, or in cooperation with Britain" (p. 353).

Voigt has consulted an impressive array of archival collections across the U.S., Great Britain, Australia, Taiwan, Japan, and Thailand as well as memoirs of the period and standard secondary monographs. Yet, there is one sin of omission. By and large, the literature on the Muslim League's increasing isolation and eventual demand for Pakistan is sparse. For example, although Hector Bolitho's study of Muhammad Ali Jinnah is cited, the *Speeches and Writings of Mr. Jinnah* edited by Jamil ud-Din Ahmad is absent—surely a more relevant title than Hitler's *Mein Kampf* or Mussolini's *Opera omnia*. It follows that the origins of the trauma of partition are lost in the steady march of chronological narrative. The conclusion admits that the political price for Indian involvement in the war was po-

larization between the Congress Party and the Muslim League and that the result of this polarization was Pakistan. But the subject is almost completely bypassed.

One minor irritation might be noticed. Although some English slogans and phrases are translated into German, others are not. Thus, we read of "Simon, go back!" propaganda (p. 30), while Churchill's noted quip is solemnly rendered: "Er nicht des Königs Erster Minister geworden sei, um den Vorsitz über den Abbau des britischen Empire zu führen" (p. 305). On balance, however, the merits of this book far outweigh its demerits. Painstaking research guarantees its place as a standard work on wartime India for the foreseeable future.

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MICHAEL ROBERTS, editor. *Documents of the Ceylon National Congress and Nationalist Politics in Ceylon, 1929–1950*. In four volumes. Colombo, Sri Lanka: Department of National Archives. 1977. Pp. ccxxii, 897; xvii, 899–1754; xvii, 1755–2697; xvii, 2699–3208. £2.50; £2.00; £2.25; £2.00.

This four-volume work of 3,208 pages is a valuable source of reference for the recent political history of Sri Lanka. The editor, Michael Roberts, has written an illuminating and insightful introduction. But there are a few important questions he leaves unanswered. Nevertheless, his analyses and the materials he has included provide a wealth of information.

The volumes, alas but quite rightly, cease in the year 1950 when the Ceylon National Congress went out of existence. But, ironically, the year marked the threshold of real, active, and meaningful nationalisms in a plural state—the nationalist Ceylon Tamil Federal Party was inaugurated in December 1949 and S. W. R. D. Bandaranaike launched his Sinhalese Buddhist-oriented Sri Lanka Freedom Party in 1951.

What, therefore, was the kind of nationalism that persisted through the period 1929 to 1950? Was it even nationalism, or was it mere constitutional agitation for further installments of liberal reform? Was there any likeness to the kinds of civil disobedience, *satyagraha*, and "August 1942" struggles launched by the great neighbor, India. Or was it a group of "brown Englishmen" seeking vainly to copy their colonial masters? With whom were the British more concerned when they ultimately relinquished power? Was there a fear that Sri Lanka's case would be taken up by India, thus depriving Britain of useful allies among the conservative bourgeois politicians of Sri Lanka? Or did the British worry more about the local Marxist movement,

which would have gained the upper hand had the local *compradore* elements failed in their quest for complete independence? In a way, it is a pity that these questions fail to attract Roberts's otherwise searching eye for detail.

"Prayers," memorials, petitions, peaceful resolutions, deputations to the governor and the secretary of state were the modest pressures imposed by the Ceylonese English-speaking trousered gentry claiming nationalist pretensions. What did they actually want? That is a question that needs adequate consideration. Certainly, they wished to have a greater proportion of the white man's jobs. More definitely, they liked political power in moderate doses. Most importantly, there was the desire to preserve the numerical superiority of the major caste group, the *goigamas*.

Roberts provides useful insights into these problems, but he is only at the fringes. For example, he refers to the Ceylon Tamil-Sinhalese *goigama* united front against the *karavas* at the 1912 election for the Educated Ceylonese seat (p. lxv). The long-term repercussions of the *karava* defeat for Sinhalese (not Ceylonese) nationalism have not been probed. Since 1950, and especially after 1956, the *karavas* have seen to it that the Ceylon Tamils have been kept in their place, *karavas* led by the F. R. Jayasuriyas, L. H. Mettanandas, and P. de S. Kularatnes and those of their ilk. Similarly, Roberts refers to the Ceylon Tamil-Sinhalese front against the Muslims during the Sinhalese Buddhist-Muslim riots of 1915 but fails to explain the reasons that motivated Tamil support for the Sinhalese. Obviously, the much praised, overrated Sir Ponnambalam Ramanathan and his more distinguished and spiritual brother Arunachalam, just as much as G. G. Ponnambalam, the ardent advocate of balanced representation in the legislature (50 percent of the seats for the Sinhalese, the balance to be distributed among the minority ethnic groups), in a later day all had hopes of becoming prime ministers in the same way as the British installed Burnham against Cheddi Jagan in Guyana by manipulating the electoral system. Likewise, it stands to the credit of the greatest Sri Lankan political general of all time, D. S. Senanayake, that as early as 1933 he discerned the signs of a Ceylon Tamil desire for enosis with India, when in an interview with a Colonial Office official "he spoke of a growing movement among the Ceylonese Tamils in favour of union with India" (p. 1,991), a view that was echoed in 1948 and is now actively being canvassed in 1978! And stranger still is the resolution moved by the Communist Party at the Congress sessions in December 1944 that "the Tamil people should have . . . the right to unfettered self-determination on its own territory, including the right of secession" in a free Ceylon.

In short, despite the tremendous labor in producing a monumental work of seminal importance, Roberts has succeeded in presenting only a prismatic view of the beginnings of Ceylonese nationalism in the period 1929-50. One is not even certain that it was genuine nationalism, let alone a type of febrile national Risorgimento. At worst it was colonial-style Englishmen striving for fringe benefits in exchange for promises of military bases and security for British commercial interests. The heart of the problem was the lively, vibrant, and virulent nationalism of the Marxists, who launched their movement in the early 1930s. If any group secured independence for Sri Lanka, it was they. Don Stephen Senanayake and his wily lieutenants cashed in on their strong and more than visible presence.

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LEE POH PING. *Chinese Society in Nineteenth-Century Singapore*. (East Asian Historical Monographs.) New York: Oxford University Press. 1978. Pp. xi, 139. \$18.95.

Despite the voluminous literature on the Chinese communities in Southeast Asia, comparatively little attention has been accorded to the predominantly Chinese city-state of Singapore, although this island has played a critical role as a regional base for Chinese activity in the surrounding territories. A particular vacuum on the social history of Singapore's Chinese has now been partially filled by Lee Poh Ping's interesting monograph, which succeeds in extending our understanding considerably. The book's title is misleading; a reader seeking an anthropologically or sociologically oriented study will be disappointed. Some aspects of conventional social history are discussed, but Lee's approach is essentially sociopolitical and focuses primarily on the Chinese elite, especially the merchants. There is considerable material here for students of Malaysian history or of the overseas Chinese, although not all scholars will agree with Lee's interpretations, sometimes articulated with more confidence than his method and analysis might warrant.

The author's purpose is rightly to challenge some earlier studies that describe a midcentury Chinese society riven by occasionally violent political instability generated by religious, dialect, and secret society rivalries manipulated by the Chinese economic elite. Lee concentrates on extracommunity influences as causal factors; his thesis is that the changing nature of the international economy impinging on the Singapore entrepôt helped to create a Chinese mercantile elite with little control over or identification with the broader Chinese society. Concurrently, the emerging British-generated "free

trade" economy stimulated the growth of often-competing secret societies as a social refuge for non-elite Chinese. The resulting violence declined only when the British colonized the Malay Peninsula in the late nineteenth century, linking both areas economically and opening new investment opportunities for Singapore entrepreneurs. The "free traders" consequently lost their pre-eminence to other Chinese more closely tied to the larger Chinese community. As secret societies declined, this new economic elite exercised power through the new dialect and clan associations. The influence of the international economy on Chinese society lessened only in the 1930s, when the depression forced many Chinese merchants to invest in local manufacturing rather than in the import-export trade.

Lee is surely correct to consider the strong influence of colonial policy and the impersonal mechanisms of the Western-dominated international economy on Singapore society; yet, there are some deficiencies. As a political scientist, his approach is more analytical than narrative; but this reviewer wishes that he had probed further into the meaning of his material and developed his theoretical perspective more clearly. The argument and supporting data need more fleshing out; the role of the international economy is insufficiently explained. Extensive lists of leaders and their characteristics comprise over 10 percent of the text; presumably, they are offered as substantiation, but Lee's analysis of them is ineffective. These lists also ignore powerful businessmen who avoided positions of public leadership.

As an avowed piece of revisionist history this work has considerable merit, but the writing style and analytical approach render the arguments less convincing than they might have been.

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GLEN BARCLAY. *A History of the Pacific from the Stone Age to the Present Day*. New York: Taplinger Publishing Company. 1978. Pp. 264. \$14.95.

A one-volume study of the history of the Pacific Ocean area from pre-culture contact until the present that professes serious intentions to scholarship is a most hazardous undertaking. The last successful attempt was Douglas L. Oliver's *The Pacific Islands* (1951), and that leaned more heavily on cultural anthropological sources than on historical ones.

Difficulties aside, Glen Barclay does manage to do a creditable job in covering a wide range of materials, although his last chapter on developments since World War II is too sketchy, being one of the book's two shortest chapters. Barclay does incorpo-

rate new anthropological and historical sources that certainly help to update Oliver's book. But the result is unsatisfactory because of the author's presentation of historical evidence. In a style of writing that might be labeled "blunderbuss modern," Barclay uses so few qualifiers that his statements must be accepted as written—and often they are questionable, outrageous, and sometimes even totally wrong.

Several examples will suffice. In discussing James Cook's landing at Botany Bay, the author states that the aborigines were "to be affected over the next 200 years perhaps more gravely than . . . any other indigenous people subjugated by Europeans" (pp. 47–48). While the statement is certainly questionable, Barclay adds to the problem by saying as part of the same sentence that the Botany Bay area "was to be made the site of one of the most affluent and probably one of the more materialistic civilizations upon earth" (p. 48). Under the heading of outrageous statements is a discussion of the Samoan influenza epidemic of 1918, which accounted for the loss of one-fourth of the population and which had been introduced through contact with a New Zealand ship. The author states that a major part of the problem was a total failure of the New Zealand military administrator to cope with the situation and adds, "It was hard to believe that human inadequacy had not been reinforced by malevolence to produce such a calamity" (p. 157). Unfortunately, the author several times makes strong statements that are just not correct, and the import of these further lessens the value of the book. He asserts, for example, that the Canadian government supported the American position in asking for an end to the Anglo-Japanese alliance after World War I. The fact was that the Canadian position had been formulated independently of the United States and presented a year earlier at the Imperial Conference of 1921. Barclay then says, "Practically speaking, it was scarcely a matter for the Canadians to concern themselves about, as they had no security problems other than those presented by the United States . . . and were not in any sense a Pacific power" (p. 170). From what source did the author glean this bit of information?

The result of this as well as other erroneous judgments is oversimplification, bias, and misinformation, making the book unacceptable as a serious investigation into an important and complex topic.

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TERESA NAGRODZKA-MAJCHRZYK. *Geneza miast u dawnych ludów tureckich (VII-XII w.)* [The Genesis of Towns among the Ancient Turkic Peoples (Seventh to Twelfth Centuries)]. Wrocław: Ossolineum. 1978. Pp. 168. 40 Zł.



The topic of this slim volume, despite the seeming incongruity of nomadism and urban development, is justifiable. The nomadic and seminomadic tribal confederations under consideration here were deeply interested in trade and the goods produced by sedentary societies. This often led to close and symbiotic relationships with the latter, particularly the towns. Having noted this (p. 9), the author, regrettably, does little to explore further this important theme. Instead, we are provided with brief political histories of the Orkhon Türks (T'u-küe), Uyghurs, Qirghiz, Türgesh, Qarluqs, Oghuz, Qarakhanids, Khazars, and Volga Bulghars together with some indication of urban developments among them as recorded in scattered Chinese, Arabic, and Persian sources. The ambiguous results of archaeological investigations are also duly noted.

Teresa Nagrodzka-Majchrzyk posits two lines of urban development among the Turkic peoples of the Eurasian steppe: seizure and evolution. The former, hardly a process of "genesis," consisted of the conquest of towns by the nomads and their gradual gravitation toward them to exploit better trade and other revenue sources. This development may be traced in the Turkicization of the oasis cities of the Tarim Basin and of the Irano-Islamic towns of Central Asia. The evolutionary mode is observable in the gradual transformation of the *orda/ordu*, the camp of the Qaghan and his immediate military retinue, into a permanent politico-administrative, cultural, commercial, and diplomatic center. The author does not, however, sufficiently differentiate between these two processes. Curiously, she has excluded reports in the sources on the towns of the Pechenegs, Kimeks, Cumans, and Chërnye Klobuki from her study. These notices provide further evidence for the evolution of the khan or chief's camp into a town.

The section on the Khazars has not gone much beyond the work of S. A. Pletneva (*Ot kochevii k gorodam* [From Nomad Camps to Towns], 1967). Missing here are the important studies of Gyula Györfly dealing with seminomadism in western Eurasia, especially the Hungarian model that was closely related to and probably derived from the Khazar model. Among the western Eurasian nomads, winter and summer quarters as well as important forts (Sarkel-Bela Vezha) were the embryos of future towns. Indeed, the importance of the winter quarters, which tended to acquire a permanent population of impoverished nomads reduced to a sedentary life, is completely overlooked. Similarly, the phenomenon of Volga Bulghar urban life, vitally linked to the fur trade and a symbiosis with the Finno-Ugrians of the forest-zone, is given only the most superficial treatment.

As for the Central Asian towns, the author cites

the long established view that the conversion of the Qarakhanids to Islam accelerated their urbanization. To this she adds that Islam was responsible for the appearance of "feudal features" in the Qarakhanid Qaghanate (p. 115). Unfortunately, we are not further enlightened on this important question nor is this thesis substantiated.

There is little in the author's general conclusions that is particularly original. Thus, we are informed, for example, that those Turkic peoples (for example, the Qirghiz and Oghuz) living at a distance from urban concentrations (hardly true of the Syr Darya Oghuz!) were less interested in organizing towns and settlements. Finally, it is noted that, even among those Turkic groups that developed an urban civilization, the roots and traditions associated with this culture were borrowed from their settled neighbors (pp. 141-42).

Despite its shortcomings, this summary of research on the origins of medieval Turkic towns will be useful to comparative urbanists as well as Turkologists who tend to ignore this important facet of Eurasian history.

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#### UNITED STATES

WILLIAM G. MCLOUGHLIN. *Revivals, Awakenings, and Reform: An Essay on Religion and Social Change in America, 1607-1977*. (Chicago History of American Religion.) Chicago: University of Chicago Press. 1978. Pp. xv, 239. \$12.50.

Scholars familiar with William G. McLoughlin's series of books on American revivalists and revivalism might expect that this slim volume would merely summarize his earlier work on Isaac Backus and revivalists Finney, Moody, Sunday, and Graham. If they failed to examine the book on that assumption, they would miss a fresh and stimulating essay that contains a provocative synthesis of recent scholarship. McLoughlin may have succeeded in that difficult task of writing a book at once helpful to graduate and undergraduate students as an introduction to issues and historiography and also valuable to scholars in the field for the interesting perspective he brings to a subject that has recently seemed quite stale.

Perhaps the most perplexing problem facing the instructor in a course devoted to the history of American religion is the difficulty of handling recent American history. Examination of any of the dozen or so texts and readers in the field shows that they are all weighted heavily toward pre-Civil War



topics and that they usually dissolve into interpretive chaos after World War I. The secularization of American society leaves these authors desperately struggling to make connections with the concerns and spiritual longings of contemporary students. Most texts seem to imply that religion is dead or else surviving only in sectarian form, no longer making any direct or significant impact upon American life. In contrast, McLoughlin finishes strong, making bold and controversial judgments about contemporary religious movements and placing these movements into the context of a theoretical framework that binds the whole of American history together.

The structure and thesis of the book can be simply summarized. McLoughlin has chosen Anthony F. C. Wallace's conception of "revitalization movements" as his central interpretive idea. After summarizing and explaining Wallace's ideas in chapter one, McLoughlin uses the schemata to interpret early Puritanism, the First and Second Great Awakenings, and the periods 1890–1920 and 1960–90(?) as possible Third and Fourth Great Awakenings. In each of these periods, McLoughlin argues, our culture passed through a crisis of legitimacy, a period of cultural distortion (characterized by nativistic or traditionalist movements and blocked "mazes"), and a final rebuilding of a new world view. Some scholars may find themselves annoyed and put off by this first chapter, but I would urge them to plunge on to see how it works in practice.

McLoughlin does not, in fact, overburden his narrative with sociological or psychological terminology. His summaries of the five eras are, in their way, elegant little historiographical essays that ably summarize the best work in the field. The recent work of Donald Mathews, Philip Greven, Rhys Isaac, John Boles, and Michael Walzer, for instance, is cited and summarized quite helpfully for the beginning student. McLoughlin's treatment of the first three eras is quite traditional, although I found his discussion of the Second Great Awakening enlightening because of the way in which his use of Wallace's concepts helps him criticize Donald Mathews's thesis of the awakening as an organizing process and to integrate discussion of the New England and Western phases of the revival. His discussion of the last two periods, 1890–1920 and 1960–90(?), will be much more controversial. There is not enough space to enable me to discuss the many issues that arise here. Whether or not one agrees with McLoughlin is not at all important, of course; what is gratifying is the appearance of a book that gives one a framework in which to discuss these issues.

ERNEST R. SANDEEN  
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PAUL F. BOLLER, JR. *Freedom and Fate in American Thought: From Edwards to Dewey*. (Bicentennial Series in American Studies, number 7.) Dallas: SMU Press. 1978. Pp. xiv, 300. \$15.00.

There can be little doubt as to the centrality of the idea of human freedom to the American experiment in self-government. Not only was the concept a justification for revolution, it was also a cornerstone of the sociopolitical theories that formed the foundation of a new nation's Constitution. But, once having established the notion of freedom as an unquestioned article of faith, few statesmen were willing to explore either its genesis or the philosophical problems that naturally arose from its acceptance. The luxury of such indifference, however, was not possible for those American thinkers who recognized how important the matter of individual freedom was to the human condition in general and to the American situation in particular. In this excellent book Paul F. Boller, Jr., has analyzed with remarkable clarity the speculations of nine men about freedom's reality or illusion.

He begins, where all such studies must, with Jonathan Edwards, that brilliant opponent of free will, considers the natural rights ideas of the revolutionary Thomas Paine, turns to Ralph Waldo Emerson's transcendental view and John C. Calhoun's more limited one, eloquently explains Frederick Douglass's personal involvement and Edward Bellamy's collectivistic vision, and concludes with essays that probe the optimism of William James and John Dewey along with the pessimism of Mark Twain. One could easily quarrel with the selection, arguing that any comprehensive study should include at least Thomas Jefferson, William Graham Sumner, Henry Adams, and George Santayana. But Boller's choices are not as arbitrary as they seem at first glance, and his book gives the reader a fair accounting of American thought about freedom and necessity at representative points during the country's evolution from colony to world power.

The book is most perceptive when it discusses those thinkers who are the least deterministic in their observations about the freedom of man. The chapters on Paine and James are good examples of precise analysis, while those examining Edwards and Twain are less satisfactory. Although repelled by Calhoun's belief that freedom was a social privilege and belonged to communities rather than individuals, Boller accurately presents the South Carolinian's arguments as he does with Emerson's frequently unstructured thoughts. The discussions of Douglass and Bellamy are sympathetic and convey the intensity of feeling each man possessed through a skillful interweaving of biography and thought.

Although the major theme developed in the book is the tension in American thought between freedom and determinism, Boller is also fascinated by the relationship between freedom and creativity. The sources of creative efforts are as mysterious as they are intriguing, and Boller is certainly correct in emphasizing that to understand that symbiosis is crucial to the whole problem of freedom. Perhaps in another book that territory will be further explored. If such a future effort matches in style and substance the high quality of *Freedom and Fate in American Thought*, our understanding of those ideas that have shaped our intellectual history will be greatly enriched.

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HILDEGARD BINDER JOHNSON *et al.* *This Land of Ours: The Acquisition and Disposition of the Public Domain. Papers Presented at an Indiana American Revolution Bicentennial Symposium, Purdue University, April 29 and 30, 1978.* Indianapolis: Indiana Historical Society. 1978. Pp. vii, 126. \$3.00.

This useful and reasonably priced volume is a collection of papers presented at the Indiana American Revolution Bicentennial Symposium on the history of the public domain, held at Purdue University in April 1978. The authors—four historians and one geographer—address significant questions and advance a number of interesting interpretations.

Hildegard Binder Johnson illustrates the cultural geographer's technique of "visual thinking" by using paintings, prints, and maps from the nineteenth century to examine mental images of the mid-western landscape. She is concerned not with the region as it actually was but with what people thought, imagined, and remembered it to be. Employing twenty-nine illustrations, she explains the perceptions and graphic techniques of several artists and cartographers. Johnson offers some fascinating insights into the meaning of this visual record, but she is less clear on the more important question of the extent to which it reflected popular thinking.

Focusing on Indiana, Malcolm J. Rohrbough skillfully tells the story of the federal land office system, which he sees as both an "exercise in agrarian democracy" and a "business" (p. 52). With rich detail and apt anecdotes, he recounts the creation of the system, describes its operation, and evaluates its success in creating a nation of freeholders. Rohrbough concludes that despite its imperfections and the abuses of many who used it, the land office system contributed significantly to the economic independence and prosperity that nineteenth-century Americans enjoyed.

Reginald Horsman's provocative essay posits a three-phase framework for analyzing scholarly work on the history of public land policy. Phase-one historians, largely under the influence of Frederick Jackson Turner, took a benign view of the policy, maintaining that it fostered landownership and agrarian democracy. Beginning in the 1930s, another group of historians, with Paul W. Gates chief among them, challenged this interpretation by arguing that malfunctioning of the policy was responsible for the excesses of speculators and monied interests and for the rise of farm tenancy. Since 1950 the interpretive direction has changed once again, as the new economic historians, interested primarily in the determinants of economic growth, have returned to a generally favorable assessment. Horsman's categories are useful for understanding the often confusing crosscurrents of public land historiography; his gratuitous and unreasoned charge that the new economic historians are "dehumanizing" history is not.

The history of Indian-white relations with regard to land transfer and ownership is the subject of Dwight L. Smith's article. Although the Indians did not understand the concept of title transfer or the use of cession treaties to implement it, these quasi-legal instruments became the principal vehicle by which Anglo-Americans wrested land from the Indians. The author's rather curious conclusion seems to excuse the use of such treaties on the grounds that they "bridged the gap between two diverse cultures" (p. 99) and, more importantly, accomplished their purpose.

In his well-crafted essay, Paul W. Gates, the dean of public land historians, claims that retention of public lands in new states as they entered the union gave the federal government an "effective instrument of nationalization" (p. 106). He provides numerous examples, especially from Indiana, to illustrate how federal land disposition shaped state internal improvement, education, taxing, and social welfare policies and affected the course of state economic development. So pervasive was this nationalizing influence, according to Gates, that it "intimately touched the life of . . . virtually everyone in the West" (p. 107).

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JOHN FRANCIS BANNON. *Herbert Eugene Bolton: The Historian and the Man, 1870-1953.* Tucson: University of Arizona Press. 1978. Pp. xix, 296. Cloth \$15.00, paper \$8.95.

Herbert Eugene Bolton, one of the giants of American history, was born in Wisconsin on July 20,

1870, received a teaching certificate at the age of twenty-two and a bachelor's degree from the University of Wisconsin at twenty-five. He completed his Doctor of Philosophy degree in 1899 at the University of Pennsylvania with a thesis entitled "The Free Negro in the South before the Civil War." After a brief teaching stint at Milwaukee State Normal, he joined the faculty of the University of Texas (1901-09), where he developed his first interests in Spanish North America. There, with Eugene C. Barker, he published his first book and began work on the important *Guide to Materials . . . in the Archives of Mexico*. He moved briefly to Stanford University (1909-11) and then took up his lifelong association with the University of California at Berkeley. In 1914 the *Guide* was published and his first Ph.D. candidate, Thomas Maitland Marshall, graduated.

From then almost to the very time of his death on January 30, 1953, he spun off books, articles, and graduate students in profusion. Through these his influence in the field of history became and remains enormous. He created the field of the Spanish Borderlands and changed forever the interpretation of United States history. Although he thought of himself as more of an antiquarian than his colleagues Walter Prescott Webb and Frederick Jackson Turner, it is likely that his contributions will be more durable.

Bolton—Webb—Turner. They made monumental contributions to the interpretation, writing, and teaching of western American history. And each inspired scores of graduate students, many of whom have made their own contributions and developed their own graduate students. But it baffles me that scholars of the capacity of Ray Billington (Turner), Walter Rundell (Webb), and John Frances Bannon (Bolton) should focus their impressive abilities and spend years of research on biographical studies of these famous historians. Is the game worth the candle (whatever that means)?

This book is clearly a labor of love, but Bannon did not sacrifice objectivity on the altar of cultism. Bolton's occasional pettiness is brought out, especially in his misdealings with Arthur Clark. And his moments of true greatness, as in his efforts to help the University of Texas even while he rejected the presidency there, are treated without undue praise. The book is well written, well organized along chronological lines, and eminently readable. The supportive scholarship is impressive, and I particularly liked the context identification of many sources—a device that keeps the reader informed without the tedious necessity of chasing the footnote citation. A lucid bibliographic essay and some footnotes are at the end of the volume. One appendix lists Bolton's published works and a second lists his graduate students by year of graduation.

Professors of history will find Bannon's book interesting and many will delight in following Bolton's trail of success through the perils of academia. Younger professors and graduate students could profit from the story of Bolton's prodigious industry and devotion to history. And some, such as myself (a Webb Ph.D. and a Barker disciple) will be titillated by references to things familiar and dear.

SEYMOUR V. CONNOR  
Texas Tech University

MARTIN MEYERSON AND DILYS PEGLER WINEGRAD. *Gladly Learn and Gladly Teach: Franklin and His Heirs at the University of Pennsylvania, 1740-1976*. Philadelphia: University of Pennsylvania Press. 1978. Pp. vi, 263. \$14.00.

This is a modest work that candidly acknowledges its limitations. The author, the president of the University of Pennsylvania, prefaces his study by specifying what is needed to supersede Edward Pott Cheyney's bicentennial *History of the University of Pennsylvania*: "an intellectual history dealing with ideas and scholarship, the transformation of education at a major American university, and a social history relating what was going on at Pennsylvania to the city of which it was a part, to student life and the life of the young in general, and to the cultural currents of this tempestuous new nation and its ties elsewhere in the world" (p. v). Having set forth these goals of the new history of education, Meyerson then avows his more limited intention to concentrate instead on selected *dramatis personae* in the university's history and thereby to investigate in microcosm significant developments in American higher education.

Meyerson's cast of characters is grouped into four "acts." Penn's colonial history is most familiar to historians, and Meyerson's eighteenth-century cast is predictable: Ben Franklin and the irascible William Smith, the two most significant of the university's founders; John Morgan and Benjamin Rush, the men most responsible for creating America's first medical school at Penn; and James Wilson, the "forgotten Founding Father" and the first law professor at the university. Next, sketches of David Rittenhouse (astronomer), Alexander Bache (chemist), and Joseph Leidy (anatomist) are intended to make Meyerson's point that the antebellum university was an American center for scientific study. In an awkward period he can only call "Transition," Meyerson then discusses the efforts of Provosts Charles Stillé and William Pepper to create a modern university at Pennsylvania in the last three decades of the nineteenth century. In the final act, Meyerson endeavors to suggest the rich complexity

of the modern University of Pennsylvania with sketches of pharmacologist Alfred N. Richards, economist Simon Nelson Patten, jurist Owen J. Roberts, architects Paul P. Cret and Louis I. Kahn, and historian Roy F. Nichols. He concludes by describing briefly the history of the university's impressive campus.

Meyerson's portraits are carefully drawn and un-failingly affectionate. The book itself is handsome, replete with evocative illustrations, and thoroughly indexed. It will, however, be of virtually no use to serious students of the history of American higher education. It is totally devoid of an analytical framework and in no way goes beyond existing histories of the University of Pennsylvania. As President Manning initially warns us, the need for a sensitive, broadly conceived history of this great institution remains.

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SHARON HARLEY and ROSALYN TERBORG-PENN. editors.  
*The Afro-American Woman: Struggles and Images.* (Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1978. Pp. xiii, 137. \$12.95.

More than a half-century ago, Arthur M. Schlesinger, Sr., published his provocative essay, "The Role of Women in American History." He concluded that their role had been generally neglected or ignored. Schlesinger seems to have been thinking primarily of white women, but his conclusion was even more applicable to Afro-Americans. This volume of essays is an attempt to counter this neglect. Its purpose is to enlighten the reader about black women "from an historical and biographical perspective" (p. viii) and to disprove the traditional assumption that their activities were restricted to those of wives, mothers, and workers.

Whatever their roles, black women found themselves handicapped by both race and sex. When seeking employment in northern factories, they encountered the racial animosity of both employers and employees. The white employers preferred to hire white women, and white women often objected to working beside black ones. When pursuing the feminist goals of the woman's movement, black women found themselves hindered by their race. The leading organizations were dominated by whites, and blacks were allowed only token or peripheral participation. Even after they formed their own associations, they continued to be relegated to minor roles in the movement generally. Within their own racial groups, they were handicapped by their sex since many black males found it difficult

to accept black females as their equals. In the case of the black woman, it was often a matter of being the "wrong" race, the "wrong" sex, or both.

One of the essays focuses on the black women of the South and concludes that they were "positive forces for change" (p. 57), in spite of the fact that they generally accepted "the roles defined for them by the larger society and by black men" (p. 57). Atlanta was a center "of constant agitation by black women" (p. 44), and Lugenia Hope, wife of the president of Morehouse College, was their leader. Among her achievements was the establishment of the Neighborhood Union, an organization that worked for the advancement of the black race through community projects. The Union came to be highly regarded and served as a model for similar organizations in other cities.

Other essays deal with black women in the "blues tradition," black women in Afro-American poetry, and black women activists. The activists selected for special consideration are Anna J. Cooper, Nannie Burroughs, and Charlotta A. Bass. Cooper and Burroughs were educators in Washington, D.C., and Bass was the vice-presidential nominee of the Progressive Party in 1952. The party made a poor showing in the election, but Bass gained the distinction of being the first black woman to seek the office of vice-president of the United States.

The contents of this volume are pioneer studies in a neglected field and show that black women do indeed have a history.

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RICHARD D. POLL, editor. *Utah's History.* Provo, Utah: Brigham Young University Press. 1978. Pp. xix, 757. \$9.95.

Designed as a comprehensive treatment of Utah from the earliest inhabitants to the 1970s and not "just another volume of Mormon history," according to its editor, this book seeks to plug existing gaps and to incorporate the latest research and interpretations. It is the product of twenty-eight contributors, eight of whom are from Brigham Young University and four from the historical department of the Church of Jesus Christ of Latter-Day Saints. Since there are thirty-six chapters, organized into three sections, the division of labor is minute.

Part one, "Utah Before the Mormon"—about one-eighth of the total—focuses on the environment, the original occupiers, the Domínguez-Escalante expedition, the fur traders, and early explorers. Part two, "From Colonization to Statehood"—not quite half of the entire text—ranges from 1847 to 1896 and emphasizes Mormon colonization, con-



frontation of beliefs and practices, and the growing "Americanization" of Utah that brought an end to plural marriage and fostered the growth of a laissez-faire economy and the separation of church and state. Part three, "Twentieth-century Utah," is the strongest portion of the book and concentrates on the decline of church domination of social, political, and economic life and the state's integration into the mainstream of the nation.

This is not a volume of surprises. It is sympathetic in its handling of the Saints. As the bibliographical essay for each chapter indicates, it is indeed well researched. It does make an effort to identify and develop neglected areas of study, especially non-Mormon, and, along with more traditional subjects, deals with unassimilated minorities, the urban sector, the role of women, the use of natural resources, and "Gentile" religion. But there is little of the "new" history here, and the chapters that strive to break fresh ground rarely show the depth and grasp of understanding apparent in some of the others.

Typical shortcomings of the multiauthor approach are visible, despite the efforts of the general editor and three associate editors to keep the stable in line. Topics are sometimes fragmented, chapters brief and choppy. Some have more detail or sophistication than others, and the writing itself is uneven. Some chapters tend toward cataloguing, but others, notably on politics since 1945, on contemporary economic development, and on natural resource utilization, are smooth, analytical, and successful.

A number of excellent photographs are scattered throughout, and there are eighteen charts and tables and twenty-three pages of maps and map explanations at the end of the volume. Careful proof-reading might have eliminated some of the obvious errors: an index not always in alphabetical order; the spelling of Cooley as "Codey" (p. 355); the substitution of "nineteenth" for the eighteenth century (p. 53); or, worse yet, the misspelling of the name of one of the authors (p. 257).

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PERRY D. WESTBROOK. *William Bradford*. (Twayne's United States Authors Series.) Boston: Twayne. 1978. Pp. 172. \$8.95.

Of all the documents of America's heroic age, none surpasses William Bradford's *Of Plimmoth Plantation* for useful, reliable information and for simple eloquence. Even more to the point for some historians, Bradford offers the archetypal statement of the New England version of "providential" history (whether Augustinian or Eusebian) and a fascinating and in-

structive case study in the use and influence of a historical text, both before and after its long-delayed publication.

Bradford and his famous history have been exhaustively cited, analyzed, and criticized. Not, however, until Perry Westbrook's little volume has there appeared a book-length study of, as its preface claims, "William Bradford as an author."

Westbrook deals with some of the main themes of *Plimmoth Plantation*—the church and community, Indian relations, the outbreak of "wickedness" and strength in adversity, and the providential theme—and devotes a chapter each to the history as a literary work and to Bradford's influence and reputation from his time to ours. He also discusses Bradford's minor writings, including the didactic and exhortative "poetry" written during the last decade of his life.

The great strength of Westbrook's study, appropriate to the Twayne series, to which the author is a veteran contributor, is its marshaling of the existing scholarship on Bradford. Since the only other book-length study is Bradford Smith's biography of 1951, this means that for the first time students and scholars can now find in a single volume summaries of or at least allusions to practically all the rather extensive scholarship and criticism that has appeared over the past quarter century. This alone, of course, makes the book worthwhile.

While Westbrook's exhaustive presentation and acknowledgement of his predecessor's work is both useful and admirable, his heavy reliance upon it occasionally proves a detriment. The chapter on the literary attributes of *Plimmoth Plantation*, for example, where a reader might reasonably expect to find the freshest and most penetrating material of all (given the book's purpose and author's credentials), turns out to be disappointingly mechanical and derivative, and in its final theoretical section, entirely dependent upon David Levin's *In Defense of Historical Literature* (1967). And, if the author had not been tied so tightly to the interpretations of others, he might have felt free to venture on his own into a typological analysis, perhaps, or a synthesis of critical approaches to the fascinating Bradford instead of merely jotting down, almost as an afterthought, hasty and approving summaries of the work in these areas of Jasper Rosenmeier and Alan B. Howard.

On the other hand, Westbrook's treatment of Bradford's theology of history allows him to take mild exception to Perry Miller's assertion that a providential interpretation was the "entire purpose" of the New England historians. This turns out to be an oversimplification in Bradford's case, and in fact Westbrook makes some interesting and original observations about the role in *Plimmoth Plantation* of



human will and the "collaboration between God and His elect."

On the question whether Bradford lost faith both in his vision of history and in the future of the Plymouth colony near the end of his life, Westbrook lines up with David Levin and against Peter Gay, Robert Daly, and, though he does not say so, Samuel Eliot Morison. The fact that *Plimmoth Plantation* runs out of steam should not lead us to conclude, says Westbrook, that Bradford had given up on either Plymouth or Providence.

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WILLIAM K. B. STOEVER. *'A Faire and Easie Way to Heaven': Covenant Theology and Antinomianism in Early Massachusetts*. Middletown, Conn.: Wesleyan University Press. 1978. Pp. xi, 251. \$19.00.

In recent years the great upheaval produced by Anne Hutchinson and her followers has received a variety of revisionist interpretations designed to reveal sociological, psychological, and even feminist dimensions to the controversy. William K. B. Stoever acknowledges but eschews such approaches and continues instead the intellectual reassessment of Puritanism begun in the 1930s by Perry Miller. Still, he strongly disagrees at fundamental points with Miller and other predecessors in this tradition of theological analysis.

The author's introduction reveals that his focus is the ministerial debate. While an early chapter covers the events of the controversy, Stoever is not primarily concerned with historical narrative. Winthrop, Vane, Wilson, and Wheelwright are given rather summary notice, secondary controversialists are ignored, and he does not appear to be deeply interested in Hutchinson herself. In contrast, he spends two detailed chapters elucidating the differences between John Cotton's emphasis on the Spirit's primacy and the answering insistence of Peter Bulkeley and Thomas Shepard on the objectivity of regenerating grace. Such discussion serves as background for a new attack on the central problem in interpreting formal Puritan thought, the exact exposition of the doctrine of the two covenants. Stoever's scrutiny of the federal system is detailed; he deals not only with New England interpretations but also with the sixteenth-century origins and developing ascendancy of covenant theology in Continental Reformed and English Puritan divinity. Here his analysis is unusually subtle, lucid, and original. The final chapters turn to Puritan preoccupation with the theological and psychological problem of assurance, where the discussion moves from the classic answers of Perkins and Ames to the

"faire and easie way to heaven" preached by those Familist radicals who, denying all creaturely activity in the drama of salvation, made divine inspiration alone suffice. Accordingly, the entire book provides an elaborate guide to the scholastic divinity that underlay the eruption in Winthrop's Boston. Only in the last chapter does Stoever venture his final assessment of that eruption.

As this summary indicates, the author sets his treatment of the Massachusetts controversy in a very broad perspective, and much of his work relates generally to seventeenth-century Reformed theology. The wide compass forces him to be selective and even somewhat arbitrary in citation, especially in concentrating on Tobias Crisp, John Traske, and John Eaton to display emergent Antinomianism, but his choices are central and instructive. Essentially, the Boston disruption interests him because it foreshadowed later Puritan divisions and because its points of controversy illuminated tensions between divine initiative and created means that were accommodated but never altogether subdued in the structure of Reformed orthodoxy. This same broad setting permits him to modify older interpretations of Edmund Morgan, Larzer Ziff, Norman Pettit, and others, and he especially challenges Miller's stress on New England's expansion of the "limits of natural ability," elaboration of "preparationism," and implication that Cotton defended an older, orthodox, and threatened emphasis on divine sovereignty. Instead, Stoever argues, Cotton was the innovator; his critics stood in the received Reformed tradition.

This study is an impressive contribution to early New England intellectual history, but it is also more. It provides insight into the evolution of Reformation doctrine, the relation of English and Continental divinity, the proliferation of sectarian Puritanism, and the disputes about the balance of nature and grace that continued in New England and post-Restoration English Nonconformity for a century. An epilogue suggests implications of the upheaval of the 1630s for some later directions of American religion extending to Edwards and Finney.

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DAVID LEVIN. *Cotton Mather: The Young Life of the Lord's Remembrancer, 1663-1703*. Cambridge: Harvard University Press. 1978. Pp. xvi, 360. \$16.50.

At least 444 printed items are believed to be by Cotton Mather, and in addition there exist a considerable number of unprinted manuscripts including the massive work he considered his masterpiece,

*Biblia Americana*. He wrote, it seems, faster than we can read, and to the sheer labor this provides for his biographer must be added the more important facts that he was descended from the first families of American Puritan thinkers, held social and political leadership, maintained a tireless interest in science, languages, history, and poetry, and occupies a place in popular mythology as the storybook bad Puritan, a gloating witch-burner and a compulsively excessive minder of his neighbor's manners. Faced with such formidable features, those studies that can be called biographical, most notably Barrett Wendell's *Cotton Mather* (1891) and Kenneth Murdock's contribution to the *Dictionary of American Biography*, have not pretended to be comprehensive but have aimed, with good success in the main, at providing an incisive profile.

This, then, is the first volume of what will surely be not only the first full biography of Cotton Mather but also the standard one. It has been prepared with such care by so painstaking a scholar that it has an immediate self-validating worth. In taking Mather to the year 1703 in this first volume, David Levin takes him to his fortieth year, far from an arbitrary point, since it was then, as Levin says, that the connection between his personal life and his country's political life ceased. The Cotton Mather of the twenty-five years yet to be reported was as passionate a minister and man as had been his younger self but was no longer connected with the political destiny of his commonwealth.

Although this is the first full biography of Mather, his times, of course, have been intensely studied in recent decades and Levin has made the major decision to accept such studies, to accept that his readers know or can find out about the larger context of Mather's activities. There is no attempt here to rewrite the times through a fuller sense of one of the period's major figures. The political squabblings after the Glorious Revolution, for example, or the circumstances of the witchcraft delusions remain unaltered by our detailed pursuit of Mather's doings; rather, what we already know of them is cited to give Mather context. We now know a great deal more about what Mather specifically did, but this knowledge does not synthesize into a picture of the man or his milieu that differs strikingly from the sketches we already have. Instead, a familiar series of images now comes to us in living color.

A second major decision that governs the work seems to be Levin's determination to maintain an interpretative point of view that explains Mather's words and acts in terms of the concepts of his day rather than to interpret this puzzling man in contemporary terms. Cotton Mather was excessive to the degree of monstrosity in almost all he did,

even though what he attempted to do was fairly conventional: his penances were extreme, to the point of masochism; his sexual anxieties were extreme; his apprehension of the pain of others as really being a lesson for him was extreme; his writings, in quantity and in their allusive rhetoric, were extreme. Within this enormous envelope, however, there was a man who was not so different from his peers save in his extremity, one that was so large that this quantitative difference became one of quality.

How and why did he become so monstrous? Such questions, raised in the twentieth century, can be answered only by accepting twentieth-century concepts. Levin, however, although fully aware of modern social and psychological theories, has opted to take Mather on essentially his own terms. To illustrate with a small point, for example, Levin recognizes the sexual dimension of Mather's physical manipulation of the body of a "possessed" woman, but since Mather did not recognize it Levin does not pursue the issue. Or, in a larger matter, since Mather felt the confusion of the witchcraft judges might have stemmed from fear, zeal, or Satan, Levin also finally lets his account of the matter rest there.

The result of these major decisions is a book detailed in scholarship and totally devoted to factual accuracy rather than modern interpretation. Those who have read Freud and Marx and Eliade and Erikson and who are reading structuralists, destructionists, and post-Marxists will not find their questions entertained, let alone answered. But they now have for their shelf of standard works on the American Puritans the first half of a detailed, scholarly biography that can be put to whatever immense service they desire.

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SACVAN BERCOVITCH. *The American Jeremiad*. Madison: University of Wisconsin Press. 1978. Pp. xvi, 239. \$15.00.

This volume is the latest in the stream of publications concerned with typology, the Puritan imagination, and the myth of America written or edited by Sacvan Bercovitch. Those who know his works will find little here that is different or surprising. As he acknowledges, *The American Jeremiad* presents "a new version" (p. xii) of an argument published earlier as a monographic essay. The earlier recension with the intriguing title, derived in part from Herman Melville, "Horologicals to Chronometricals: The Rhetoric of the Jeremiad," was also published by the University of Wisconsin Press. Portions of *The American Jeremiad* have appeared in

scholarly journals, including the *American Quarterly*. Sometimes an argument is more persuasive when repeated; sometimes it seems strained.

Bercovitch—one of the more creative interpreters of the literature of early America—tells us again that the national myth owes much to the rhetoric of the New England Puritans. The jeremiad, he maintains, became the archetypal expression in form and content of the “notion of American exceptionalism” (p. xiii). The first generation of Puritans shaped the genre and the doctrine, thereby laying the foundation for an emergent American ideology and national orthodoxy. By combining lamentations on the present with a vision of the future, the preachers of the jeremiads contributed to the process of Americanization and to the subsequent development of middle-class culture. The Puritans bequeathed to their heirs and to the society a forward-looking concept of self that, Bercovitch declares, distinguishes America qualitatively from other modern nations. Only in America have the sacred and the secular been fused.

In this volume Bercovitch traces the fortunes of the jeremiad as rhetoric and ritual into the mid-nineteenth century. He finds that the leaders of the second and third generations in New England continued to combine the roles of the critic and of the visionary in their political sermons as they exhorted a prodigal society to return to covenant responsibilities. Nonetheless, the ultimate outcome was never in doubt, for the New World, as also ancient Israel, had been assigned a unique role in the divine economy—an assumption the Puritans derived from Biblical precedents and prophecies. In the eighteenth century, the base of the mission was broadened by shifting from Biblical to other grounds. The millennial reflections of Jonathan Edwards and other evangelicals, according to Bercovitch, focused upon the “American” character of Canaan; the pronouncements of the clerical Whigs centered upon secular history. So the jeremiad played a major role in the war for independence. Throughout the century the same formula persisted: crises pointed beyond themselves to future glories. Even the Federalist spokesmen invoked the same ritual, urging restraints upon the new society as they anticipated the success of the American republic. This “figural mode of consensus” (p. 153), Bercovitch contends, was a legacy from the Puritans. The Romantics also joined the tradition. The principal writers of the American Renaissance were unwilling to abandon the jeremiadic pattern—striking evidence, in his view, of the continuing impact of the notion of a national covenant.

This analysis may not do justice to the subtlety of Bercovitch’s argument, which draws heavily upon insights from various disciplines. On the other

hand, some of his refinements are clouded by the format and style of the volume. *The American Jeremiad* has the marks of a scriptural commentary: canonical texts interpreted in a primary discourse with more than one hundred secondary glosses and marginal excursions as well as a full complement of backnotes. The reader is often left to integrate these diverse materials. Bercovitch assumes the role of the exegete, seeking to exhaust every level of meaning in the texts, the literal and historical as well as the symbolic and figural. Historians will flinch when parts of the chain are forged. The links need to be tested to determine whether the perceptual consensus existed in the minds of the speaker and his audience or only in the imagination of the gifted exegete. To paraphrase an apocalyptic canon, “Predated prophecy is not always good history.” The millennial reflections of Jonathan Edwards, for example, appear unable to bear all that the commentator has placed upon them. In addition, the idea that the “anti-jeremiad” (p. 191), constructed by those who rejected the symbol of America, can always be inverted to become yet another statement of consensus seems painfully strained. (In general, the role of cultural dissent is not treated adequately.) And the continuing use of “horological” and “chronometrical” as analytical categories tends to obfuscate rather than clarify issues; “profane” and “sacred,” by contrast, have much to commend their use. Finally, at times the reader must wonder if Bercovitch has himself followed the preacher from exegesis to application, from analysis and commentary to exhortation and celebration, thereby joining the jeremiadic tradition in America.

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A. LEON HIGGINBOTHAM, JR. *In the Matter of Color: Race and the American Legal Process, the Colonial Period*. New York: Oxford University Press. 1978. Pp. xxiii, 512. \$15.00.

A. Leon Higginbotham’s *In the Matter of Color* is a definitive study of racism, slavery, and the law in early America and is the first volume of a projected series on the legal position of blacks in the United States. The book first examines statute law and court cases in six representative American colonies (Virginia, Massachusetts, New York, South Carolina, Georgia, and Pennsylvania). Higginbotham concludes his book by analyzing the *Somerset* case, which outlawed chattel slavery in England, and the implications of the Declaration of Independence for blacks.

Higginbotham, a federal appeals judge, writes with moral intensity and constantly reminds read-

ers that the law depersonalized both enslaved and free black people. He contends that the legal status of blacks progressively declined during the eighteenth century throughout mainland British North America. At first, white Englishmen expressed ambivalence about the legal position of blacks. Court cases and laws in places as diverse as Virginia and Massachusetts placed slaves somewhere between indentured servants and chattel slaves. As the proportion of blacks in the population rose, slave laws became more severe. Slaves were treated as property rather than as people in all the colonies Higginbotham examined, but slave laws were far worse in the South, where masters and white patrolers could whip, maim, and even kill slaves with impunity. Nothing could prevent the spread of the slave system. Settlers in Georgia, for instance, began to develop a plantation system in the 1740s despite the legal prohibition of slavery, and the law eventually caught up with this reality.

The book adds to our knowledge of the development of white racism in the colonies and extends the work of such scholars as Winthrop Jordan and David Brion Davis. Law and the colonial courts, Higginbotham contends, did not merely reflect and justify racism but helped to expand its influence. Whites could both maintain power over their slaves and act in a perfectly legal manner because the law prevented slaves from protecting themselves, their families, and their homes. Several colonies, for example, punished whites for unprovoked attacks on blacks but did not allow slaves to testify against whites and permitted whites to defend themselves by merely stating that they were provoked.

Higginbotham defines law narrowly as statutes and court decisions and interprets these materials strictly. This method obscures the actual legal status of slaves. The law for most slaves was not found in the statute books or enforced in courts, but determined by a nearly absolute master. Every colony eventually codified its laws about slaves, but the legal position of slaves continued to change long after the law ceased to develop. In Virginia, for instance, the slave law was codified by 1705, but crucial changes in the status of blacks occurred in the decades between 1720 and 1750, when native-born Afro-Americans began to dominate the slave population. The relationship between masters and slaves became increasingly ambivalent over time: masters realized that slaves were people similar to themselves but continued to insist that legally they were merely property.

The book infers the actual status of blacks in society from law and court cases and usually ignores the economic and demographic base of slave society. Recent work by Michael Mullin, Peter Wood, Russell Menard, Gary Nash, and this reviewer,

among others, suggests that the position of blacks in a place depended heavily on the composition of the slave population and the uses of slave labor. A slave who lived in Philadelphia in 1765 probably had a smaller chance to develop a satisfying family and community life than a slave who lived in Virginia at the same time, despite the greater severity of Virginia's laws, because slaves in Virginia lived longer and resided on plantations. Despite these problems, Higginbotham's book remains the best discussion of slave law; scholars and students who want to understand the relationship between law and society will have to turn elsewhere.

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BRUCE C. DANIELS, editor. *Town and County: Essays on the Structure of Local Government in the American Colonies*. Middletown, Conn.: Wesleyan University Press. 1978. Pp. xiv, 279. \$20.00.

The provenance of this volume is obvious. Over the past decade more and more early American social historians have concerned themselves with life at the local level. The most fashionable master's and doctoral thesis subject of the moment is undoubtedly a local study of some sort. Editor Bruce C. Daniels and the eight historians joining him quite consciously set out to make available an introduction to the diverse terminologies and institutional structures along the Anglo-American seaboard. And, despite some unevenness among the essays, all succeed in this purpose—David Thomas Konig and Daniels himself on New England; Nicholas Varga, Wayne L. Brockelman, and Judith M. Diamondstone on New York and Pennsylvania; Lois Green Carr, Robert Wheeler, and William H. Seiler on Maryland and Virginia; and Richard Waterhouse on South Carolina. Each essay offers a quick initiation to the uninitiated.

One suspects, however, that Daniels, at least, wanted more. In his preface he promises an examination of "the dynamics as well as the structure of local government" and writes of the necessity of linking "the new social history" with classic institutional history (p. x). In his introduction he poses broad questions relative to both the diversity and similarity of local government along the coastline. Why diversity? What significance should we impart to similarities? If preface and introduction are to be the criteria by which the essays are judged, they must be labeled—with one exception—prosaic. They neither fulfill the promise nor answer the questions.

The one exception is Konig's "English Legal Change and the Origins of Local Government in



Northern Massachusetts." Cutting through what he calls the "Puritan gloss" (p. 13), Konig locates the roots of New England's local government in pervasive English fears of social turbulence and in a resulting English movement to structure authority in such a way as to curb it. Lois Carr quietly seconds Konig when she cites "order," "well ordering," and "quiet rule" as "key words in the thoughts of English and Maryland rulers and ruled" (p. 74). Daniels himself notes in his introduction a natural consequent of Konig's argument—magisterial power—as a common element in the diverse structures along the coast. In his own contribution, however, he ignores the point in favor of a traditional stress on the unique Puritanism of Connecticut.

In the final analysis, one is disappointed in the book. True, it will serve as an introduction to the uninitiated in early American local history, but even they will ultimately be forced to the classic institutional histories. There is simply not enough room in brief essays to cover the necessary ground, while a poor index makes the volume a difficult reference. Those already deep in the subject will be excited only by Konig's contribution.

DARRETT B. RUTMAN  
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M. H. SMITH. *The Writs of Assistance Case*. Berkeley and Los Angeles: University of California Press. 1978. Pp. xii, 589. \$30.00.

This book is an excellent specimen of "lawyer's history." Indeed, it illustrates two distinct subdivisions of the discipline, "legislative history" and minute scrutiny of litigation strategies. Of wider interest, it provides background for what has been considered an important prerevolutionary event. As the author (a lawyer in the British civil service) makes clear, the writ of assistance was not a search warrant. It facilitated customs inspections by enlisting the aid of local peace officers and making disobedience a contempt of court. The author's law is both informative and sound, although he may have missed a constitutional grievance when emphasizing that the writ was not returnable. Though that fact has been misunderstood by historians, it is generally irrelevant. The writ was issued at common law, and, though not returnable, the decision to issue its companion writ of restoration (ordering property wrongly seized by customs officers restored to the owner) and the judicial determination of "probable cause" were made at vice admiralty, an unprecedented (and therefore unconstitutional) joinder of common and civil law.

The book says everything that can be said about the writs of assistance case and then repeats it. The

reader is overwhelmed by detail. M. H. Smith was indefatigable, tracking down every possible source to answer every possible question. As a result, he does much guessing about matters of little importance, such as the precise wording of a warrant. He also draws inferences that can be challenged. Massachusetts's royal governor was motivated by private profit when promoting the writs, true enough, but the purpose was not to open up colonial warehouses. It was to shield imperial customs men from *trespass* liability by surrounding them with local peace officers who hopefully would be joined as defendants.

There is one very significant benefit to the mass of information that extends this book to about three times its required length. Although almost buried by detail, the reader who wades through the trivia emerges with a new perspective concerning one of our prerevolutionary giants. By simply providing so much information about the law, Smith reveals James Otis's ineptness as a constitutional lawyer. A master of pleading and procedure though he may have been, the man simply did not understand the imperial constitution. The fact is of great importance as it means that much of what Otis said is irrelevant. It no longer matters, for example, whether he (or Edward Coke) meant to enunciate a doctrine of judicial review or was only stating a position of strict statutory construction. Smith's evidence leaves no doubt that Otis compounded the seventeenth-century English constitution with the constitution of the revolutionary settlement. Otis's statements must be taken with qualification, therefore, not only because he was arguing forensically as an advocate (not a statesman) but also because he was arguing anachronistic law. It has been thought that the eighteenth-century constitution of parliamentary supremacy shackled Otis. More accurately, he was as bad a constitutional lawyer as he was a historian and did not understand that the constitution of Coke was not the constitution of Mansfield. The book is well printed, with footnotes at the bottom of the page and fifteen appendixes of documents, yet marred by extensive use of slang.

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MICHAEL KAMMEN. *A Season of Youth: The American Revolution and the Historical Imagination*. New York: Alfred A. Knopf. 1978. Pp. xxi, 384. \$15.00.

Michael Kammen's thesis is that the American Revolution serves as the foundation of our national tradition. He looks at a multitude of materials that, he writes, when "taken singly are unremarkable. Considered as a mosaic, however, they turn out to



reveal profoundly important patterns" (p. xvi). He analyzes sculpture, painting, drama, poetry, and novels that expressed themes of the Revolution from the 1770s to the 1970s. He chooses these materials because he believes that "we can never fully know the intricacies through which a society weaves a knowledge of its origins and development; but we are likely to learn more from the gossip of popular culture than from the gospel of academe" (p. xv).

This culture, for Kammen, has defined the Revolution as a rite of passage. This organic metaphor carries the meaning of an inevitable process of becoming independent. This has meant, he writes, that "Our authors have been rather conservative in their social outlook; and the net result has been to derevolutionize the American Revolution" (p. 211). The novelists, by "presenting our revolution as the national rite of passage, made it seem historically unique and non-replicable. One comes of age only once" (p. 219).

While appreciating Kammen's research, analysis, and conclusions, this reviewer came away from the book feeling confused. One reason is Kammen's refusal to take a clear stand on his understanding of cultural categories. If the artistic materials discussed in the book are popular culture, they are the popular culture of a dominant Anglo-American elite. How does he distinguish between the novels and the histories produced by that elite culture? Many historians would not be surprised if one said that a dominant convention in our historical writing has defined "our Revolution as the national rite of passage" and therefore "made it seem historically unique and non-replicable."

Starting national tradition so abruptly with the Revolution, Kammen avoids confronting the problem of relating the Revolution to the Anglo-American tradition, which finds the metaphorical foundation of the nation in the Puritan errand in the wilderness. He does not tell us what his disagreements are with the extensive scholarship of the last decade, which makes this link. It is interesting in this respect to compare his book with Sacvan Bercovitch's *The American Jeremiad* (1978) and Peter Karsten's *Patriot Heroes in England and America: Political Symbolism and Changing Values Over Three Centuries* (1978).

Kammen also does not clarify the grounds for his belief that there was a strong egalitarian aspect to the Revolution. He implies that this egalitarianism is a constant "reality" that endures from 1776 to the present. And he criticizes the "imaginative writers" who "have been perceiving the Revolution as a rite of passage for so long now that that tradition, that perception, has become virtually as important as the original Revolutionary realities" (p. 220). But he does not tell us why we should consider the egalitarian

tradition as more real than the rite-of-passage tradition. Furthermore, he places the rite-of-passage tradition within the social and economic patterns of our history while lamenting the conflict between the "egalitarian ethos" and "a society that in fact became ever more stratified" (p. 37). One wonders, then, how he hopes to revitalize the "real" egalitarian tradition of the Revolution in the face of the more powerful rite-of-passage tradition which, according to his analysis, expresses the reality of a hierarchical class structure.

DAVID W. NOBLE  
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CARL J. VIPPERMAN. *The Rise of Rawlins Lowndes, 1721-1800*. (Tricentennial Series, number 12.) Columbia: University of South Carolina Press, for the South Carolina Tricentennial Commission. 1978. Pp. xiii, 276. \$14.95.

"He was originally a Parish Orphan Boy, nor knows his own Origin—Taken from the Dunghill by our late Provost Marshal—Made his Valet—then learn'd to read and write—then became Goaler—Then Provost Marshal—Got Money—Married Well—Settled Plantations—became a Planter—A Magistrate—A Senator—Speaker of the House and now Chief Judge" (p. ix). So wrote Charles Woodmason, itinerant Anglican minister, reviewing the career of Rawlins Lowndes on the eve of the Revolution.

Carl J. Vipperman does not quarrel with this sketch, believing it "essentially correct" (p. x). But he does argue that the "unknown origins" statement needs correction; the sketch needs elaboration; and Lowndes's career post-1769 needs discussion. Vipperman attempts to meet these goals in compiling this narrative biography of Rawlins Lowndes, in spite of the "dearth of personal papers" (p. x).

In Vipperman's work, there emerges a person determined to succeed in establishing wealth and name among the Carolina gentry, despite the scandal of a father who "blew out his Brains" (p. 18) when Rawlins was in his early teens. Lowndes did, of course, rise from his lowly estate as a ward of the provost marshal to wealthy landowner and president of South Carolina, March 12, 1778 to January 20, 1779.

Despite Lowndes's eighteenth-century "rags-to-riches success story" repeated too often on the dust jacket and here and there in the book, it appears clear that Lowndes had more than his fair share of family, marital, and political connections to assist him at crucial moments in his career, from the time of his first post as deputy provost marshal to assem-

bly speaker, chief judge, and finally, president of South Carolina. Not that he was without talent, but his talent usually appears more modest than extraordinary. Nevertheless, he did have the ability to apply himself diligently to his affairs, once he attained a desired position; this trait more often than not accounted for his success. When war crisis demanded courageous and innovative leadership, however, he foundered, and "withdrew from active participation" in governmental affairs for the duration of the Revolution (p. 222). He returned briefly at the end of the 1780s to oppose, as sage and seer, the ratification of the Constitution, but this anti-Federalist activity was without effect.

Vipperman displays a thorough familiarity with the sources necessary to an understanding of South Carolina's colonial history in his presentation of Lowndes's career; frequently, the author offers sound insights into colonial legal history, gained from the scrutiny of the records of the provost marshal's and judge's offices for the period of Lowndes's tenure. This elucidation of legal history in late colonial South Carolina is the primary value of the work. It is questionable whether tracing the career of Lowndes was, in fact, the best vehicle for such a study.

Other than its contribution to the legal history of South Carolina, the book adds little new data to, or interpretation of, standard colonial-revolutionary history. Further, it is difficult to ascertain what it was about Lowndes that merited this biography, since he is often submerged in Vipperman's pedestrian narration.

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GEOFFREY SEED, *James Wilson*. Millwood, N.Y.: KTO Press. 1978. Pp. viii, 229. \$16.95.

The dust jacket subtitle for this book, "Scottish Intellectual and American Statesman," implies more than the book is; a more precise title would be "James Wilson: His Political Thought, Illustrated by Important Events in His Career." The book is not a new biography of Wilson (happily not needed, because a good one by C. P. Smith already exists), but rather is a succinct, judicious, well-written analysis of Wilson as a political thinker. In fact, the book is notably deficient in its sense of the context of Wilson's career, and there is no effort even to outline the events of his life. What little there is about his family, his business enterprises, and even his political activities is incomplete and superficial. Properly forewarned, however, this is no liability

because Geoffrey Seed's book is well worth reading for its exposition of Wilson's thought.

Seed rightly concentrates on four brilliant and influential years, 1787-90, when Wilson played a dominant role in the Federal Convention (matched only by Madison), gave public currency to the basic arguments for ratification (his printed speeches being far more widely known in 1787-88 than *The Federalist Papers*), carried the day for the Constitution in the Pennsylvania Ratifying Convention, took his seat as an associate justice of the United States Supreme Court, and was the guiding spirit behind the much-admired Pennsylvania Constitution of 1790. In those years Wilson, more than any other person, worked out the theoretical grounds for joining the republican principle of consent with positive, active government. Thus, as Seed observes, Wilson accepted Hamilton's emphasis on energy unalloyed with his skepticism of popular will and also accepted Jefferson's fidelity to that will without his skepticism of centralized authority. The crux, in Wilson's oft-used metaphor, was a "pyramid government" resting firmly on the wide base of an extensive suffrage (Wilson always sought removal of restrictions, and his logic pointed irresistibly toward the "one person—one vote" doctrine) but rising to a broadly empowered unitary executive. This concept led Wilson to advocate the popular election of the president to strengthen both his authority and his direct responsibility to the people and to envision a chief executive capable of wise leadership in the public interest. "The great desideratum of politics," government both efficient and free, Wilson argued, could be achieved "only by forming a popular government. To render government efficient, power must be given liberally; to render it free as well as efficient, those powers must be drawn from the people, as directly and as immediately as possible" (p. 134). Wilson further explained this insight in arguing that the governor of Pennsylvania be unrestrained by a council: "By the whole state this single executive magistrate will be chosen; *for* the whole state he will be appointed; *to* the whole state he will be responsible; *toward* the whole state he will be impartial" (p. 138). Wilson thus shared with his colleagues in nationbuilding a firm conception of the need for a patriot leader above party and special interests, but more than most of them he grasped how this ideal might be enhanced rather than undermined by enlarging popular participation. He thus may have been, as Seed proposes, the most profoundly democratic of all the Founding Fathers. Seed quotes Lord Bryce approvingly: "Wilson's . . . mind, broad, penetrating, exact, and luminous, . . . gave [his] nation at the outset of its career, sound, just principles for the conduct of its government, principles which are in harmony with its

character, and are capable of progressive expansion as it expands" (p. 179). Though this may display an uncommon Scottish extravagance, it is nonetheless gratifying to have, on the bicentennial of the flowering of the Scottish Enlightenment, this perceptive study by a Scottish scholar, citing the preeminent Scottish interpreter of American government, of the most distinguished Scotsman among the Founding Fathers.

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JAMES H. BROUSSARD. *The Southern Federalists, 1800–1816*. Baton Rouge: Louisiana State University Press. 1978. Pp. xiii, 438. \$24.95.

Studies of the Federalist Party after 1800, such as those by David H. Fischer and Shaw Livermore, focus primarily on northern Federalists. James H. Broussard set out to remedy this imbalance with a study of southern Federalists after 1800, and he has succeeded admirably. He has asked all the right questions—Who were the southern Federalists? What were their beliefs? Where did they live? What influenced their voting behavior? And his answers are thoroughly researched and judiciously constructed.

Southern Federalists were a tiny minority without effective organization or charismatic appeal. They reacted to events, rather than influenced them, and their political fortunes seemed to depend on national crises. They are a narrow and uninviting subject for research, but Broussard somehow transcends them. By carefully analyzing the roots of Federalism, he indirectly reveals much about Republicans and, indeed, about southern politics in general.

Southern Federalists, Broussard concludes, were not the spokesmen for commercial, urban, or upper-class interests. Their opposition to the Jefferson administration was broad based and responsible. They criticized Jefferson when they thought he was wasting money (as in the construction of naval gunboats) and supported him when they felt he acted in the national interest (as in the Louisiana Purchase). On matters of local interest their record was mixed. They generally opposed suffrage reform, but their record on slavery and public education was somewhat more progressive than the Republicans'. Neither party had any set attitudes on the role of government in the economy. Legislative voting on bank charters, toll roads, canals, and river improvements reflected local interests. Southern Federalists, in short, were not a regional wing of a national party organization. In fact, they had very little contact with northern Federalist leaders. They were

simply a collection of like-minded men who reflected the interests and attitudes of their localities.

Broussard makes a determined, but not wholly successful, effort to find out what those special interests and attitudes were. He takes up one by one the various hypotheses that have been offered in the past—that Federalists were a wealthy or mercantile elite, that they were split by generational tension, or that they represented agricultural backwaters. None satisfactorily explains southern Federalism.

The one common factor in southern Federalism, according to Broussard, is fear of Napoleonic France. It seems likely that this, in turn, was a function of ethnicity. Counties that were most persistently Federalist in voting behavior often had high concentrations of Germans and Scots. Both groups had good reason to side with Britain in the European war, and that could have translated into Federalism in America. Broussard suggests this possibility, but he does not pursue it. It is the one flaw—a minor one at that—in an otherwise splendid book.

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NORMAN K. RISJORD. *Chesapeake Politics, 1781–1800*. New York: Columbia University Press. 1978. Pp. xiv, 715. \$27.50.

In *Chesapeake Politics*, Norman K. Risjord argues that modern political parties in Maryland, Virginia, and North Carolina originated from the debtor-creditor conflicts of the postrevolutionary period and that a two-party system on both state and national levels evolved from these local clashes of interest to reach full maturity in the election year of 1800. Since Risjord takes as his premise that the physical features of "the Chesapeake" influenced postwar economic alignments, his first chapters survey the historical geography of the region. He uses computer analysis of the debtor-creditor divisions to show how groups formed in the state legislatures with a cohesion rate of 75 percent or more according to recorded roll calls. He then argues that the legislative clusters that formed in the first Congress around questions of public credit were extensions of these earlier local divisions. And in a final section he examines the further development of these clusters from 1793 to 1800. Risjord concludes that the great foreign policy issues agitated after 1793 did not change but only reinforced existing alignments, though he concedes that they did help to politicize constituencies outside the legislature, namely, the executive officers and the people.

Risjord's approach is behavioral: he describes what politicians did. He devotes 572 pages to de-

scriptions of the political behavior of each state's legislature and congressional delegation and of each state's response to the national elections. Much of this material has been touched on in secondary works, but Risjord supplies a more detailed account than any other writer has given. This is particularly true of his passages on legislative politics in the states. Indeed, his text often takes on the character of the legislative journals on which he relies. The presence of so much detail will frustrate those eager to get to the point, but Risjord's method requires that he examine and submit to his analytical matrix as much political behavior as possible. Though sometimes tedious, this approach can also be rewarding. For example, Risjord gives by far the fullest account of the relationship between state politics and national politics during the formative period of the first party system, a subject that the secondary literature has generally neglected.

Nonetheless, I found the book disturbing in its ultimate implications. Risjord never makes explicit what he thinks his material means. The volume ends abruptly, with an assertion that Maryland politics had reached maturity by 1801, when the Republicans took over from the Federalists. But Risjord did not write a book of this length, nor would anybody read one, for so slight a conclusion. As I understand it, the unstated meaning of his material is that the Revolution expressed "the genius of American politics" in giving birth to modern political parties based more on local interest and organization than on common ideology and revolutionary commitment. Risjord imposes the present on the past to make this point. He argues that the high degree of cohesion he finds, particularly at the end of the 1790s, can be explained only in terms of political organizations that, like modern parties are essentially concerned with place and local interest. He goes out of his way to demonstrate that ideology and its expression in public opinion had very little to do with the shaping of alignments.

I believe that Risjord's unwillingness to conceive of any politics but the kind that exists today renders him unable to understand a great bourgeois revolution and leads him to trivialize the very achievements I think he means to celebrate. In the context of other revolutions, both past and present, the true "genius of American politics" has surely been the nation's success in developing institutions that enable it to resolve most conflicts without violence. It is a pity that a failure of historical imagination should have prevented Risjord from seeing that the Founding Fathers succeeded in doing this even where the fundamentals of republicanism and the meaning of the Revolution itself were at stake.

RICHARD BUEL, JR.  
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MURPHY D. SMITH. *Oak from an Acorn. A History of the American Philosophical Society Library, 1770-1803*. Wilmington, Del.: Scholarly Resources. 1976. Pp. 291. \$15.00.

This work is an unusual history of a remarkable library, now more than two centuries old, formed and developed for its first three decades from gifts alone, and from its inception designed as a resource for scientists and men of science. Initiated primarily by Benjamin Franklin and associates who formed the American Philosophical Society, the library constituted one dimension of their endeavor to keep the American colonies informed of scientific progress and achievements throughout the world. Soon the library became the primary means of communication with other learned societies in Europe through exchange of scientific papers and publications.

With no permanent home of its own until Philosophical Hall was built in 1789, the library was housed for a time in Carpenter's Hall, then in the home of the society's first curator, David Rittenhouse, and for the next century and a half in Philosophical Hall. Lack of space finally forced the society to transfer its volumes to the nearby Drexel Building. When that structure was demolished, the library went into temporary quarters. In 1952 the society began construction of the present library building on the former site of the Drexel Building—where, in fact, the Library Company of Philadelphia had stood from 1789 to 1884. In its new quarters the library's holdings of more than one hundred and sixty thousand volumes, innumerable pamphlets, prints, and maps, and several million manuscripts serve scholars and students throughout the world.

The author is associate librarian of the American Philosophical Society and has been in charge of its archives and manuscript collections for more than two decades. With his intimate knowledge of the library's history and holdings, he is eminently qualified to produce this most useful resource, which will serve not only users of the collections but also historians of science and of American civilization in particular.

Murphy D. Smith has divided his work into two parts. The first and larger section presents a detailed account of the acquisition of publications from the arrival of the first pamphlet and a broadside in October 1770 through March 18, 1803, when the first official librarian, John Vaughan, was appointed. The second section of the book provides an alphabetical listing of the acquisitions for the same period.

The first curator of the collections, David Rittenhouse, was formally named as the first librarian



early in 1775 as well, and during the early period of the library's growth the collection of publications, enclosed in two bookcases ordered for the purpose in 1773 and 1775, was maintained in his house. After the Revolution it was housed in Carpenter's Hall, where the Society held its meetings. Following the acquisition of a suitable site in 1785, the construction of Philosophical Hall proceeded slowly, and not until early 1790 was the building sufficiently complete for the society to move in with its natural history collections and library. At first the society was equally concerned with assembling a collection of natural curiosities, a subject with which the present work is not concerned.

Following two chapters of general history, Smith provides a chronological record, year by year, of each of the 932 items acquired by 1803, with descriptive information about them and a running account of the activities of the society. An excellent extensive index makes the volume readily useful.

This volume is an invaluable reference work for anyone working in the history of American science in the eighteenth century, for not only does it reveal another intimate dimension of the history of the growth of the American Philosophical Society but also provides a useful reference to those works in the sciences which were available in the colonies and new nation in that period.

SILVIO A. BEDINI  
Smithsonian Institution

H. BENJAMIN POWELL. *Philadelphia's First Fuel Crisis: Jacob Cist and the Developing Market for Pennsylvania Anthracite*. University Park: Pennsylvania State University Press. 1978. Pp. 167. \$10.00.

This volume disappointingly treats an important subject. The work evidences little familiarity with the processes and wider currents of American business and economic developments during the early nineteenth century, and its level of analysis is weak at best and juvenile at worst. The material is presented in a narrow and eulogistic fashion that is antiquarian and descriptive rather than analytical and informed. As a particular economic and business history, it is mistitled, misconceived, superficial, repetitious of both fact and idea, overstated, given to psychologizing Cist's behavior, and, on one occasion at least (p. 96), supports an assertion with contradictory factual material.

Some of these deficiencies stem from its organization. The 148 pages of text are divided into 8 chapters sandwiched between a four-page introduction and a three-page conclusion. The chapters are uneven in scope as well as substance. The best one (Seven: "Anthracite and Pennsylvania's Internal

Improvements") is also the longest (23 pages) and demonstrates H. Benjamin Powell to be better at politics than economics. His shortest chapter (Eight: "Cist's Ultimate Dream: An Anthracite Railroad to New York City") is merely 7 pages long, though it ranks in quality with chapter seven. Probably the worst chapter is five ("Evolution of the Wyoming Valley Coal Trade"); the material in it relates only tangentially to its title.

Essentially, Powell fails to deal adequately with two of the most important aspects—as I perceive them from the information presented here—of Cist's career. First, Powell does not provide us with a detailed description of the Philadelphia coal market from the War of 1812 to 1825, the period when Cist's career flourished. We need to know production costs; it is not sufficient to be told that blacksmiths, for example, favored anthracite because it burned hotter, longer, and with less sulfurous residues (and was, therefore, more "economical") than soft coal; we need to know the dollars and cents of it as producers did. The change from firewood or soft coal to anthracite did not occur because producers "liked" hard coal better, it paid them to do so; and the factual material in this volume supports the notion.

More seriously, Powell does not seem to understand the economic processes of America's "transportation revolution" (George Taylor's book is not even cited in Powell's notes). As Powell shows, Cist was much more consciously a merchant than a miner, and one dimension of his career was concerned with making transportation improvements that would allow him to market his anthracite. But Powell does not give us a systematic economic analysis of what happened. Instead, and this is typical throughout the volume, we are given isolated instances of Cist's marketing profits (for example, in 1815 Lehigh anthracite cost \$9 per ton and was selling for \$16–18 per ton in Philadelphia [p. 77]). Rather than approaching the impact of transportation improvements upon the anthracite trade from a market perspective, Powell uses Cist's individual one, which achieves, as usual, fragmentary results.

Finally, something must be said about style. I would not object if the prose were merely inept, but it is ungrammatical as well. Surely, writers should know about paragraph construction and the differences between "while" and "though," "since" and "because," and "which" and "that."

GARY L. BROWNE  
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Baltimore County

MARY K. BONSTEEL TACHAU. *Federal Courts in the Early Republic: Kentucky, 1789–1816*. Princeton: Princeton University Press. 1978. Pp. ix, 234. \$16.50.



This valuable monograph attempts a systematic study of the federal court of original jurisdiction in one state during the twenty-seven-year tenure of its first judge. Mary K. Bonsteel Tachau has assembled, coordinated, and interpreted the extensive though cryptic records of this court. After clarifying the various statutory changes in the court's jurisdiction, the author provides a biographical sketch of the central character, Judge Harry Innes, and brief sketches of the other personnel of the court: clerks, marshals, and United States attorneys. One chapter is then devoted to the procedural law of the court and four chapters discuss the substantive law, dealt with under topical headings: enforcement of federal revenue laws, other criminal cases, property cases, and other private civil suits.

Tachau is at her best when she fits the workings of this Kentucky federal court into a broader historical and social context. The sketch of Judge Innes's background, for instance, is ample enough to give the reader not only a grasp of the bare details of his legal education, reading, and experience but also an appreciation of Innes as a person. Tachau shows him engaged in an intense, long-standing feud with a leading family of political rivals (the Marshall family with its ties reaching ultimately to the Supreme Court). She portrays his elitist, moralizing philosophy as well as his strong localist sentiments. Innes's momentary flirtation with Spanish officials is fitted into its context to make intelligible how he and others could consider separating Kentucky from the Union and allying it with Spain to assure access to the Mississippi. Tachau suggests that Innes's treatment of the many cases that came before him to enforce the federal tax on whiskey betrayed his strong sympathies with the local moonshiners and that his treatment of British creditors showed his willingness to ignore the amount in controversy limitation on his court's jurisdiction.

The weakest portions of the book stem from Tachau's obvious impatience with the "almost incomprehensibly ritualistic" "legal technicalities," the "archaic procedures and practices" that make up so large a part of the legal records of this early court. Anyone who has struggled through such records can appreciate the difficulty. The legal historian, however, must understand and explain with precise accuracy these forms of pleading and process; such were the bricks and mortar of which this court was built. Here Tachau often fails. It appears anomalous to praise Judge Innes repeatedly for his care to assure due process of law and then to neglect the "legal technicalities" that Innes probably considered crucial to due process. Innes's admonition to counsel should have alerted Tachau to regard these "technicalities" as decisively important to a full understanding of this early court. "The Latitude con-

tended for by Mr. Attorney," Innes said, "goes at once to destroy that System of Good pleading which has stood the test for Ages past and which I hope will continue to be strictly attended to by Judges." The long, tedious research leading to an otherwise fine book on this early federal court has been seriously marred by Tachau's failure to approach the processes and procedures of the court with at least as much understanding and appreciation of their importance as Judge Innes had.

HENRY J. BOURGUIGNON  
*University of Toledo*

MALCOLM J. ROHRBOUGH. *The Trans-Appalachian Frontier: People, Societies, and Institutions, 1775-1850*. New York: Oxford University Press. 1978. Pp. xiv, 444. \$17.50.

At the outset this book appears to retell the familiar story of the settlement of Turner's "New West," but the reader soon realizes that Malcolm J. Rohrbough's treatment offers new insights and a deeper understanding of the process than previous studies. The author carefully restricts this book to the frontier phase of the trans-Appalachian region, saying that when the frontier had passed the area was merely rural.

Rohrbough's framework for studying this frontier serves well, for the most part. He considers the Kentucky-Tennessee area between 1775 and 1795, then shifts attention to the area north of the Ohio River during the same period, showing how settlers challenged the wilderness with a society based on Anglo-American institutions of law, religion, education, and farming. Next he considers these areas similarly during the next twenty years as the institutions took root. The following fifteen years, 1815-30, saw sizable migrations into both regions that tempered the process of settling the frontier. At this point, the highly instructive comparative treatment gives way to separate chapters on frontier conditions in Michigan, Florida, and Arkansas. The author is doubtless correct in positing that these areas offered sufficiently different situations to warrant individual chapters, but with this change in approach, the book loses some subtlety of interpretation and structure. Between 1830 and 1850, a second great migration filled lands in the deep South vacated by displaced Indians and in the unoccupied areas in the Old Northwest, transforming these frontiers into farms.

Throughout the book Rohrbough develops several themes with great skill. As Americans crossed the Appalachians into the new country, they carried a heritage of centuries of English jurisprudence. This concept of the law and governmental structure

formed the foundation for frontier society. Often, as in Dade County, the Florida Territory prescribed a court system when the entire population numbered barely enough to comprise a jury. Along with government came taxes—sufficient to make perpetual squatters out of some frontiersmen as they sought to escape the pernicious effects of government. Another theme the author traces adroitly is the agricultural basis of the frontier. Although this holds no surprises, Rohrbough handles the *agricultural interrelation of the economy* convincingly.

A great strength is the book's comparative quality. The author demonstrates the differences between establishing governmental machinery in Ohio and the Kentucky-Tennessee region. Arthur St. Clair certainly had the support of the national government in his efforts to administer the Ohio Territory, but that government was in the uncertain process of transition from the Articles of Confederation to the Constitution and offered little help. By contrast, the well-established state governments of Virginia and North Carolina lent concrete aid to their fledgling counties in the West. The book also vivifies the frontier experience through vignettes of representative settlers, such as Gideon Lincecum, Lovira Hart, and Thomas Dabney. Most of those mentioned have left just one footprint on the sands of time, but Rohrbough establishes the import of their small contributions to a new society.

The author bases this book on deep and expert research into a wide variety of sources: manuscripts and archives, theses and dissertations, and published secondary literature. Apparently, the only relevant publication that escaped his intensive search was Robert Hay's work on the celebration of the Fourth of July on the frontier. Some dozen maps enhance the quality of this significant volume, as does a clear literary style. The conclusion that "enormous abundance" and "repeated successes" typified this frontier is not supported by the text. More moderate claims would have harmonized better with the evidence so impressively presented.

WALTER RUNDELL, JR.  
*University of Maryland*

RICHARD CARWARDINE. *Transatlantic Revivalism: Popular Evangelicalism in Britain and America, 1790-1865*. (Contributions in American History, number 75.) Westport, Conn.: Greenwood Press. 1978. Pp. xviii, 249. \$18.95.

Richard Carwardine has added another dimension to our growing recognition of the relationship between nineteenth century British and American social movements. In 1963 Frank Thistlethwaite pub-

lished his pioneer work on the Atlantic community, which was followed by more specialized studies of evangelicalism, temperance, and antislavery. Carwardine has studied revivals.

This work corroborates many previous conclusions about patterns of interdependence. As in the temperance and antislavery movements, there was a steady exchange of correspondence, publications, and agents (evangelists). British revivalists borrowed many American techniques: the "anxious seat" and "altar call," prayer circles operating in advance to promote psychological readiness, and camp and "protracted" meetings to sustain enthusiasm. In both countries Sunday Schools were consciously utilized to prepare young people for revival conversions; Bible classes helped to train itinerant preachers. Britain contributed directly to American revivals by supplying immigrants who helped to meet the demand for preachers, teachers, and tract distributors; British publishing houses produced most of the early revival literature. Carwardine supports the conclusion that British antislavery conviction "reinforced the element of social morality within American revivalism" (p. 32). Such pressures may have pushed Southerners to more fervid revivalism in order to demonstrate that they, too, were the recipients of God's blessings.

One of the most interesting theses is that the British, with a traditional distaste for excesses of emotionalism and more restrained by high church authority, helped to moderate American revivalism, thus making it acceptable even in relatively sophisticated urban centers. British influence was also apparent in the growing emphasis on an educated ministry, although, to begin with in Britain and America, a revival's strength lay in achieving rapport between the people and preachers who were of them, not above them. Carwardine believes that revivals contributed to an atmosphere of egalitarianism even in class-conscious Britain. Greater equality for women was an inevitable product of their participation in revival activities, leading (albeit infrequently) to female preaching.

Carwardine agrees that the insecurities growing out of industrialization helped to shape the form and intensity of revival cycles; but he found no direct correlation between revivals and economic depressions or unemployment. On this point his statistical charts are helpful. Correlation of revivals with political instability is equally uncertain. He concludes that the major factor in the psychology of revivals was within religion itself: "a climate of high expectancy" which could be cultivated by preachers and which was reinforced by interdenominational rivalry. True, to heighten emotions, preachers might capitalize on guilt feelings by interpreting depressions as God's punishments, but revivals have a

momentum of their own, evident in times of prosperity too.

There are chapters on leading evangelists (Finney does not get his usual top-billing). Carwardine emphasizes urban more than rural revivals, with some comparisons of revivals among the British industrial proletariat and those of frontier America. It is a readable book, packed with both information and analysis which heighten our appreciation for the role of revivalism in shaping Anglo-American society and culture.

BETTY FLADELAND  
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E. BROOKS HOLIFIELD. *The Gentlemen Theologians: American Theology in Southern Culture, 1795-1860*. Durham, N.C.: Duke University Press. 1978. Pp. x, 262. \$14.75.

Challenging a widely held impression that religion in the Old South was virtually devoid of rational content or strivings, E. Brooks Holifield directs our attention to a group of pre-Civil War ecclesiastical intellectuals whom he calls "gentlemen theologians." Though few in number (Holifield specifically identifies one hundred), they preached to elite town and city congregations, held prestigious faculty positions at colleges and universities, wrote carefully reasoned books and articles, and exerted a considerable influence upon their respective communities. None can be reasonably acclaimed as a giant among the world's religious thinkers, yet they do rank as creditable exponents of systems of thought that also flourished among sophisticated northerners and Europeans. Orthodox almost without exception, they were invariably preoccupied with proofs of "the reasonableness of faith and the rationality of the world" (p. 206). Like their parishioners, students, and readers, they abhorred "the religious antics and social ineptitude of the unwashed and untutored" (p. 206), never doubting that their own refined perspectives were "congruent with the deepest nature of things" (p. 206). Holifield feels that these men have been unduly neglected because of a fascination on the part of scholarly writers with Southern circuit riders, exhorters, camp meetings, pietism, proslavery polemics, Biblicalism, and the like.

One major chapter provides a discussion of Scottish Common Sense Realism and its interpreters in the region; others cover pronouncements on moral philosophy, theodicy, the meaning of the sacraments, and objective evidences that reinforced religious faith. The focus alternates mostly among Presbyterians, Lutherans, Episcopalians, Roman Catholics, Methodists, and Baptists. Notably, less

than half the sample was affiliated with the ascendant Methodists and Baptists, whereas a majority of the group held orders in the numerically weak Presbyterian and Episcopal churches. This is appropriate, however, in view of drastic differences in level of education between clergymen of the former and the latter. Southern Methodists had no theological seminary before the Civil War, and the Southern Baptist Theological Seminary did not begin functioning until the late 1850s.

There is little effort to delineate the tendencies of Southern religion vis-à-vis religion in the North and in Europe, and there is little mention of two issues that perhaps most engaged rank-and-file sectarians—the correct mode of baptism and whether regenerate saints invariably continue in a condition of grace. But the volume does provide an authoritative, wide-ranging study of clergymen who were both erudite and orthodox, which is what the author set out to do. This reviewer fully agrees with an assertion on the book's cover that "Professor Holifield has opened important new vistas on southern theology, and [that] his penetrating exploration demands serious attention."

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THOMAS VIRGIL PETERSON. *Ham and Japheth: The Mythic World of Whites in the Antebellum South*. (ATLA Monograph Series, number 12.) Metuchen, N.J.: Scarecrow Press and American Theological Library Association. 1978. Pp. xiii, 181. \$9.00.

"God shall enlarge Japheth, and he shall dwell in the tents of Shem, and Canaan shall be his servant." In Genesis 9, Thomas Virgil Peterson explains, antebellum white Southerners found the authority for their conviction that Japheth, Shem, and Ham (Canaan's father) were the progenitors of the white, red, and black races in America. According to their imaginative interpretation, God's command sentenced the descendants of Ham to slavery, to inferiority, and to blackness. Moreover, the story of Noah and his three sons provided divine sanction for patriarchal and conservative ideals of government. Ham's primary offense was his assault on the authority of the family, presided over by God's chosen representative, the patriarch. Hierarchical control by those best suited by nature to rule demanded the authority of masters over their slaves, wives, and children. Through the Ham story, slavery was included within the structure of the patriarchal family. Patriarchal and paternal slavery became both a punishment for Ham's sin and a benevolent institution for his descendants, a system in which blacks could be both cared for and made

productive. Thus, Genesis 9 “symbolically framed the ethos of plantation life within sacred history” (p. 117).

Peterson’s purpose is not merely to discuss the uses of the Bible to promote slavery. Rather, he presents the ambitious thesis that the story of Noah’s curse functioned as a “persuasive cultural myth” (p. 12) and, indeed, that the Scriptures became the “foundation for the society’s institutions, values, and beliefs” (p. 7). The major problem of this brief study, done originally as a dissertation in American religious studies at Stanford University, is its failure to evaluate theology in relation to other forces that gave legitimacy to the slaveholding way of life. His argument rests upon a narrow base of evidence—some one hundred or so published contemporary references to the Ham story—and an esoteric analysis of the applicability of competing theories of myth by Geertz, Lévi-Strauss, Eliade, Ricoeur, Janeway, Voegelin, Malinowski, and others. Only through the private papers of individual Southerners, however, can the significance of religious ideas, economic advantage, class bias, racial fear, psychic need, and personal experience be probed and evaluated. Without that integration and comparison, Peterson is suggestive but not persuasive. We can be certain only that the Ham story was published widely and that some creative whites used it to link slavery, racism, paternalism, patriarchy, and conservatism.

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CLIFFORD E. CLARK, JR. *Henry Ward Beecher: Spokesman for a Middle-Class America*. Urbana: University of Illinois Press. 1978. Pp. 288. \$12.95.

The Reverend Henry Ward Beecher (1813–87) was pastor of Plymouth Congregational Church in Brooklyn, New York, from 1847 until his death. Son of the Reverend Lyman Beecher and brother of Catharine Beecher and Harriet Beecher Stowe, he was a member of one of the best-known American families of the nineteenth century, a family of preachers, teachers, and reformers. For thousands of Americans he was no less than a modern oracle. Historians have not, however, been kind to him. Most know little about him, and most of what they do know, or believe, is unflattering.

Several eulogistic biographies appeared shortly after Beecher’s death. The best of these is the one by his disciple, Lyman Abbott, published in 1903. In 1927 Paxton Hibben produced a “debunking” biography that pictured Beecher as a buffoon and a hypocrite and became the standard work. Following in Hibben’s footsteps, Robert Shaplen in 1954

wrote a short book entitled *Free Love and Heavenly Sinners*, a narrative of Beecher’s allegedly adulterous relationship with the wife of Theodore Tilton, a friend and associate. In 1970 William G. McLoughlin of Brown University, a distinguished authority on American religious history, produced *The Meaning of Henry Ward Beecher*, a study of Beecher’s thought as revealed in his voluminous published works, with special reference to the period 1840 to 1870. In 1973 came Jane Shaffer Elsmere’s scholarly study, *Henry Ward Beecher: The Indiana Years, 1837–1847*.

Now Clifford E. Clark, Jr., has given the historical profession a book that can be recommended confidently to professors, students, and the public as the best biography published to date. It has grown out of his dissertation in American Studies completed at Harvard in 1968. Clark has examined with great care not only Beecher’s published writings but also relevant manuscript collections, among which the most important one is the Beecher family papers at Yale University. He has done a superb job of making some sense out of Beecher’s frequently contradictory statements.

Henry Ward Beecher’s chief importance was the part he played in the final repudiation of the harsh tenets of Calvinism in the United States and in the emergence of what came to be called “evangelical liberalism.” He preferred to emphasize God’s goodness and mercy rather than His role as judge. He preached the immanence of a benevolent God in nature, human beings, society, and history and was the first important American clergyman to come to terms with the theory of evolution.

In comparison with most other ministers, Beecher was also ahead of his time in his advocacy of social reform and in his preaching on political issues. He used his pulpit to ransom the freedom of several young slave girls and suggested that Sharp’s rifles might be more effective than Bibles in saving Kansas from the curse of slavery. He was an active supporter of the newborn Republican Party. He toured Great Britain during the Civil War to drum up support for the Union cause and took a keen interest in the problems of Reconstruction. He was also active in the temperance crusade and the woman’s rights movement.

Beecher’s custom of speaking out freely on public issues occasionally got him into trouble, as, for example, during the railway strikes of 1877, when he remarked that “a man who cannot live on bread and water is not fit to live” (p. 236). Liberal historians have pounced on this unfortunate statement as proof that he was not a “Christian evolutionist” but a Social Darwinist. Clark notes that by 1882 Beecher had become “willing to accept the organization of labor and the use of strikes” (p. 266).



The judicious quality of Clark's work is evident in his treatment of the court case of *Tilton v. Beecher*. "Given the contradictory nature of the evidence," he writes, "it was at the time and is now impossible to judge whether Beecher had actually committed adultery" (p. 224). The case ended in a hung jury, nine to three in favor of Beecher's innocence.

Clark's book is well documented with ample footnotes at the end of each chapter and is well written.

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BUELL E. COBB, JR. *The Sacred Harp: A Tradition and Its Music*. Athens: University of Georgia Press. 1978. Pp. ix, 245. \$10.00.

In 1844 B. F. White and E. J. King, two Georgia singing teachers, published a book of religious choral music which they called *The Sacred Harp*. Hundreds of similar tunebooks appeared in nineteenth-century America, but none showed the staying power of White and King's work. Revised and reissued frequently, it is still in print today. Moreover, the book has given its name to a living tradition—a social and religious subculture that defines itself through music.

Buell E. Cobb, Jr.'s *The Sacred Harp: A Tradition and Its Music* is a worthy successor to the works of George Pullen Jackson, whose *White Spirituals in the Southern Uplands* (1933) first called attention to the harmonized folk hymnody of the South. Cobb, a professor of English and a Sacred Harp singer himself, is well qualified to write an "inside" history of the movement. Drawing on personal interviews, archives, and secondary literature, he describes in detail the tradition as a whole, its music, its early history, the editions of *The Sacred Harp*, and the rise and decline of the big singing conventions. If, as the final chapter explains, the outlook for the movement's future is uncertain, the author has too much respect for its adherents to sentimentalize about its possible disappearance. As Cobb puts it, "the fact that students at Harvard or Berkeley find this music engaging will not help much if the boys and girls in Haleyville, Alabama, or Tallapoosa, Georgia, are not interested in learning to sing it" (p. 160).

Cobb is an enterprising scholar; his narrative is engaging and well written. Best of all, he lets the singers speak for themselves in numerous well-chosen quotations that help illuminate the way of life that has supported *The Sacred Harp* for over a century.

Two appendixes round out the book. The first is a virtual "union list" of some six hundred traditional singings held each year, including dates and locations. This collation of dozens of smaller listings affords a truer picture of the extent of the tradition

than has previously been available. The second, a selection of forty pieces from *The Sacred Harp*, seems an afterthought. A note explaining why these pieces were chosen and a separate index to them would have been welcome.

DAVID WARREN STEEL  
RICHARD CRAWFORD  
University of Michigan

TODD L. SAVITT. *Medicine and Slavery: The Diseases and Health Care of Blacks in Antebellum Virginia*. (Blacks in the New World.) Urbana: University of Illinois Press. 1978. Pp. 332. \$15.00.

If only the regular antebellum physician could have tucked this volume into his saddlebags before setting out to combat disease among slaves on Virginia plantations. It would have told him that on the clinical level almost all of his cherished procedures ranging from venesection to massive dosing were more likely to kill than cure. Thus his competitors—Thomsonians, slave masters, the slaves themselves—were all much more successful than he, if for no other reason than they did less harm.

But the book would also confirm many of his medical suspicions regarding blacks. Many slaves were "naturally" resistant to malaria, whereas whites were not. They were also resistant to yellow fever but more susceptible to cold injury, cholera, and possibly pneumonia. In the case of most susceptibilities, Todd L. Savitt finds environmental as opposed to innate predisposing factors at fault and, indeed, in an introductory discussion of black and white medical differences concludes that, "for the most part, the illnesses and treatments of blacks were identical to those of whites" (p. 47).

Thus death rolls reveal a much higher frequency of lethal cholera among blacks than whites in part because residence and occupation put slaves at greater risk of infection and in part, as our mounted physician knows, because he and his colleagues tended to falsify morbidity and mortality records to spare whites the stigma of having hosted a "lower class disease." Savitt claims that they did the same with infant deaths supposedly caused by the mother overlaying the child and smothering it during sleep, for black infants monopolized this cause of death in Virginia. Here Savitt makes an important contribution by marshaling evidence to argue convincingly that many, if not most, of these deaths were in reality today's Sudden Infant Death Syndrome or "crib death."

Slave clothing, quarters, diet, and working conditions are all scrutinized by Savitt to explain why slaves suffered from a host of diseases common to the period ranging from typhus to "tobacosis," the tobacco workers' "black lung." Moreover, he does not limit himself to plantation slaves but looks at



urban slaves as well. Nor is the book confined to black physical ailments; Savitt also examines mental afflictions, as witnessed by a fine chapter on insanity and its treatment. The work concludes with a poignant look at "Blacks as Medical Specimens," a "situation [that] existed as a result of the degraded condition of blacks, both free and slave, in Virginia" (p. 307).

It is difficult to be critical of this work, which brings Savitt's unique combination of medical and historical training to bear on a wealth of primary materials unearthed by his dissertation research in more than a score of archives, libraries, and private collections. The study should leave the specialist well satisfied. The author's failure, however, to address larger themes may leave general readers, particularly those with an eye on the debate over slave well-being, a bit perplexed as to the meaning of the book.

For example, Savitt's dissertation upon which this study is based was entitled "Sound Minds and Sound Bodies: The Diseases and Health Care of Blacks in Ante Bellum Virginia." Does this "soundness" image (which permeates the present work) add credence to the argument recently advanced in some quarters that slave health was as good (or as bad) as that of antebellum whites, at least in Virginia? And what does Savitt's portrayal mean in light of the black postbellum crisis of health when the Afro-American death rate soared far above that of white Americans?

Another potential difficulty with the study is that readers may be tempted to generalize for the entire South on the basis of Virginia—a temptation that Savitt inadvertently encourages when he states in his afterword (p. 312) that the health "situation in Virginia typified conditions throughout the pre-Civil War South." Yet this is a treacherous assertion. Savitt himself points out that for the time span he is considering, Virginia had a stable slave population, that is, "there was little black immigration into the state" (pp. 2–3). By contrast, much of the South, particularly the New Cotton South, during this period saw a great deal of immigration, bringing blacks into new disease zone environments, where they confronted new pathogenic strains not to mention the special health problems of any frontier people created by housing, sanitation, and limited food supplies. Virginia, on the other hand, was much more urbanized, and therefore many of its slaves suffered to a far greater extent from diseases such as tuberculosis than did their rural cousins. Finally, Virginia was in the wheat belt, which meant many nutritional advantages were available—advantages not enjoyed by most slaves, whose principal, and indeed frequently only, cereal was corn.

These objections, however, are minor in the light of Savitt's accomplishment. His work stands as the

first to examine systematically slave health and antebellum medical conditions on a state-wide basis in the light of current medical knowledge. It is therefore an important effort that should both stimulate and stand as a model for other such studies.

KENNETH KIPLE

VIRGINIA KIPLE

Bowling Green State University

HERBERT HOVENKAMP. *Science and Religion in America, 1800–1860*. Philadelphia: University of Pennsylvania Press. 1978. Pp. xii, 273. \$16.00.

Upon scanning a book such as this, most readers will think it contains a survey of how theologians and scientists interacted for fifty years during the early national period. It is more ambitious than that, taking a larger framework from 1770 to 1870 as its real time span and discussing topics relevant to science and religion from a viewpoint of critical philosophical scrutiny that judges ideas in both spheres. The author is especially good in sketching cameos of various spokesmen in their particular settings. His ability to depict specific persons and issues is not matched, however, by an overall breadth of vision or sense of historical development. This volume actually falls within a genre of American letters begun during the Enlightenment and perfected by Andrew Dickson White in his *History of the Warfare of Science with Theology in Christendom* (1876). Other than provide us with extensive details regarding a chosen time period, it hardly improves the content of this genre or expands its perspective. Historians interested in the topic would do better to consult Theodore Dwight Bozeman, *Protestants in an Age of Science* (1977) for religion and Charles Coulston Gillispie, *The Edge of Objectivity* (1960) for science.

Beginning in the 1770s, a preponderant number of American intellectuals adopted Scottish Realism as a viable means of comprehending the world around them. With a stress on empirical evidence, they pursued a vision of unified knowledge, one that would comprehend information about natural phenomena, human experience, and Biblical teachings in a unified, well-integrated system. But, the author insists, Protestants were bent on combining knowledge and belief so thoroughly that they would achieve a religion free from all doubt, a scientific theology. From two premises—that God created nature and inspired the Bible—molders of opinion pursued the ideal of synthesizing mutually reinforcing data drawn from both fields. The book's early chapters demonstrate how they hammered out key epistemological definitions of evidence, hypothesis, metaphysics, and factual knowledge. But unified thought was doomed, we are told, because religion

as a pseudoscience held perversely to propositions that could not stand up to variant findings, especially those in geology and biology. The book's latter chapters display this tension through various topics. Since the original relationship is judged to have been illegitimate, this entire exhibition is conducted with a condescending, mocking tone. Strains between doctrinaire anachronisms and fresh investigation finally reached an open rupture when Darwin published his *On the Origin of Species* in 1859. It is the author's conclusion that orthodox Protestants were those deeply and sincerely committed to a position that grew increasingly untenable. As unfettered science progressed, religious spokesmen became more close-minded, accepting what ultimately proved to be indefensible irrationalities.

It is questionable whether such an overview represents all Protestants or accurately discerns priorities among a century of scientists, but on a more modest, procedural level the book will be unsettling to most readers for another reason. It is very difficult to decide whether the author wants to reflect the ideas of each thinker in turn or to find fault with them. He clearly wants to provide a running commentary on religious and scientific inadequacies as he proceeds, and thus the work bears a remarkable similarity to another genre characterized by Frank Hugh Foster's tendentious monument, *A Genetic History of the New England Theology* (1907). Readers must be wary of accepting any conclusion about a nineteenth-century spokesman because it might stem from nothing more than the logic of a twentieth-century philosophical position. In the last analysis, historical understanding is often juxtaposed with judgmental commentary, and one cannot trust that the resulting prose reflects ideas from the historical context. This study does not actually distort history to serve a preconceived set of interpretations, but we still need to look at thinkers who combined religious and scientific ideas in their writing, asking what they were trying to accomplish within limited contexts and why.

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DON HARRISON DOYLE. *The Social Order of a Frontier Community: Jacksonville, Illinois, 1825-70*. Urbana: University of Illinois Press. 1978. Pp. xiii, 289. \$12.50.

This is a welcome addition to the slowly growing number of scholarly histories of smaller American cities. Don Harrison Doyle offers Jacksonville as a study of the *urban* frontier, and that is also welcome. Earlier polarities, though they may once have helped focus descriptive history, are increasingly

seen as impediments to analysis: "urbanization" need not mean smokestacks and high-rise parking lots, and "frontier" comprises much more than a spreading scatter of family farms.

Platted in 1825, Jacksonville grew from a few hundred people in 1830 to over five thousand in 1860. Unlike better-known, and certainly less typical, western settlements, this one did not "take off" into commercial expansion, industrialization, and political notoriety. Defeated in a contest over railroad location and divided by religious and political controversies, Jacksonville's leading families ultimately gave up metropolitan ambition and settled upon the goal of being another "Athens of the West," a cradle of refinement and education in the country's agricultural heartland.

Doyle stresses community imagery, voluntary associations, ethnic and political divisions, and the growth of public authority. His aim is to analyze problem-solving in a new, changing, and demographically fluid setting; the types and functions of social conflict in such a setting; and the effort to formulate and sustain an idea of community through the whole process. His study of boosterism is highly suggestive and may encourage deeper investigation of that important phenomenon, too often taken for granted. Especially interesting is Doyle's brief account of the "inversion" of boosterism when the ambitions for growth were more or less consciously given up. A desperate struggle to secure the location of the state university failed in 1867, and the local ideologists turned to producing an image of a deliberately small but genteel community. They began to build "a tradition of local history that justified small-town failure as chosen success" (p. 255).

I wish he had done more with the development of local history traditions; for this purpose, his cut-off date of 1870 is a bit too early. Considerably more on the origins and pre-Jacksonville histories of his boosters and city fathers would also have been valuable. Some readers may be put off by what will appear to them as an excessively theoretical first chapter; let them start at chapter two. These criticisms aside, the book is a worthy contribution to our study of the early materials from which arguments over the nature of "community" have grown.

A. THEODORE BROWN  
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Milwaukee

JAMES E. DAVIS. *Frontier America, 1800-1840: A Comparative Demographic Analysis of the Settlement Process*. Glendale, Calif.: Arthur H. Clark Company. 1977. Pp. 220. \$15.50.

James E. Davis begins his study with the warning that the West is "a difficult to define region, one

which was always in motion, and one in which numerous forces—some external, some internal, ever combining and ever changing in relative importance—were part of a process that operated in an organic fashion over space and through time” (p. 28). Rather than attack or defend the Turner hypothesis, however, Davis has chosen to analyze “some of the demographic characteristics of the individuals, households, and groups that were lured or shoved into the West” between 1800 and 1840. By studying seven demographic variables—household size, household composition, age, sex, race, the condition of blacks on the frontier, and occupation—and by comparing these variables in the settled Northeast, the frontier Northwest, the settled Southeast, and the frontier South, he gets at obvious and subtle distinctions in the four sections. He also compares the northern and southern frontiers in different census years. His findings are based on the census reports and local records of 252 counties, defined as frontier or settled, that ranged in an arc from Georgia to Michigan in the years 1800–40.

Many of Davis’s carefully researched findings tend to corroborate existing impressions. His assertion that this cis-Mississippi frontier was one of great abundance, “whose resources were privately exploited in a comparatively sustained triumph of man over nature from 1815 onward,” is scarcely surprising. But Davis argues that the fruits of forest and soil were so easily available that survival did not require a large family work force and therefore did not put a premium on childbearing as a source of labor. On the other hand, nearly everyone lived in a household unit with others. Practically no one lived alone. Further, the household unit owned and operated most of the means of production and distribution and “was the basic economic unit on the frontier” (p. 20). Contrary to the belief that frontier families were large, Davis finds they were not notably larger or smaller than those found in the settled portions.

Davis’s analysis also turns up some other facts that both frontier and family historians should find intriguing. His hypothesis that the northern frontier had less typical frontier traits than the southern frontier turned out to be untrue. Yet by 1840 the family household size on the northern frontier was smaller than the household size in the East, while the southern frontier family was larger than that found in the Southeast. Still the differences between pioneer and eastern households were so minor, he concludes, that “there is little reason to believe that frontier personality and society were remarkably different, at least insofar as the differences were products of difference in household size and composition” (p. 99). Similarly, although frontier regions did contain more youths and males than the East,

the older notion that they consisted primarily of males and “loners” does not appear to be true. Using statistical methods carefully and illustrating his findings with maps and charts, Davis’s often qualified and careful conclusions do seem to hold up under scrutiny.

As to the racial characteristics of the two frontiers, Davis finds very few blacks on the northern frontier (only 0.3 percent in 1840), while the number on the southern frontier fluctuated wildly, jumping from 10 percent in 1800 to 30 percent in the 1830s and then declining to 13 percent in 1840. There is no evidence that the frontier ever provided freedom or opportunity to blacks, slave or free. In terms of occupational patterns, no frontier was *more* agrarian than the settled Northeast in 1820, while the settled portion of the Southeast was less agrarian than the northern frontier by 1840.

There are some methodological problems and some omissions. Davis himself points out that many manuscript census records for the early southern frontier have disappeared; consequently, his conclusions about that area are often based on narrative accounts. On the other hand, while he is careful to analyze breast-feeding as a form of birth control—and therefore an explanation for smaller families on the frontier—he does not discuss the role of disease in keeping frontier families as small as their eastern counterparts. There are also relatively few examples of how a frontier family functioned either economically or socially.

Though not a definitive study, *Frontier America* is a valuable book in which the author asks significant questions and provides important corrective new information. The first chapter is a penetrating, even epigrammatic, essay on the nature of the frontier. In the more statistical ones he never makes claims he cannot substantiate. He is to be commended not only for the scholarly way in which he has used demography to get at this process but also for laying out promising methods of further investigation.

HOWARD R. LAMAR  
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M. LELYN BRANIN. *The Early Potters and Potteries of Maine*. Middletown, Conn.: Wesleyan University Press. 1978. Pp. xvi, 262. \$22.00.

There appears to be a rising interest in the United States in artifacts of various kinds from the past, and the past need not be terribly remote. Where there is interest in a subject, books will be written on it, and M. Lelyn Branin’s book shows that there is an interest in pottery produced mainly by small shops operated by one or two men in the United States in the last century.

Branin has produced an exhaustive list of potters who worked in Maine from the earliest settlement until about 1900 and of their potteries. He has not dealt with modern potters. His work is arranged by towns but excellent indexes and appendixes make it simple to find individual potters. By using wills, estate inventories, deeds, census records, tax records, local and county histories, atlases, and newspapers, he has assembled an amazing amount of material, mostly biographical. He may not be able to tell us much about the pottery made by an early potter, but he can often tell us where he was born, who his parents were, who he married and what the names of his children were, what land he bought, what offices he held in the town, and so on, for such is the information the records contain. We must certainly praise Branin's industry and care. The list seems accurate and complete, and accuracy and completeness are features that similar books for other areas I have consulted seem to lack.

The book will probably appeal mostly to collectors of Maine pottery, for Branin makes few attempts to generalize on the basis of the data he has assembled. Such questions as the relationships between potteries of Maine and other areas, the general economic and social factors that account for the decline of potting, and the position of the potter in society are not dealt with directly. Nor is pottery itself really dealt with. The kinds of pots listed in early records are given, but no attempt is made to explain what they were like and how they were used. Stew pots are listed and illustrated, but we are not told how they were used, for example. Were they used for cooking or serving stew, or both? Were they used at a fireplace or on a stove? It should be emphasized that Branin set out to answer one set of questions and answered them well. He should not be criticized because he did not answer others. His book, however, has limited appeal; perhaps he will write another that will be broader. One final note: colored illustrations would have made the book more appealing and, of course, more expensive.

WARREN E. ROBERTS  
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WILLIAM H. PIERSON, JR. *American Buildings and Their Architects: Technology and the Picturesque, The Corporate and the Early Gothic Styles.* (American Buildings and Their Architects.) Garden City, N.Y.: Doubleday. 1978. Pp. xxviii, 500. \$12.95.

William H. Pierson's *American Buildings and Their Architects* is a measure of American architectural history's coming of age. Nothing on this encyclopedic scale has been attempted in the field before. Unlike

most encyclopedic efforts, however, it is not appearing in chronological order, but hops back and forth: *The Colonial and Neo-Classical Styles*, then *Progressive and Academic Ideals at the Turn of the Twentieth Century*, then *The Impact of European Modernism in the Mid-Twentieth Century*, now back to *Technology and the Picturesque, The Corporate and Early Gothic Styles*. Why the linkage of "corporate" (that is, early factory) and Gothic Revival? As the author explains, "the time span of both developments coincided exactly with that of the Greek Revival"—roughly from the 1830s to the 1860s. Furthermore, "the confrontation between a nature-oriented romanticism on the one hand and a burgeoning technology on the other . . . formed the dynamic core of nineteenth-century America." In fact, no effective connection between the two is established; the book consists of a short section on early factory architecture and factory towns, with particular emphasis on Lowell and Harrisville, and a long section on the Gothic Revival. Both are very good; but it is, as the author is first to admit, an awkward arrangement. Why he has felt so bound to a sectioned chronological system of presentation is not apparent, because he seems entirely aware of the symbolic and associational aspects of Gothic Revival as well as its formalistic development—indeed, this is among the book's greatest merits.

The heart of the book is the long study of St. Patrick's Cathedral in New York by James Renwick, which includes a reconstruction of the original plan of St. Patrick's as envisaged by Renwick and the extraordinary Lady Chapel he planned for its east end. Pierson makes a convincing case that the most important architectural descendant of St. Patrick's was not a Roman Catholic church, but the Episcopal cathedral of New York, St. John the Divine, designed to be the fourth largest cathedral in the world in 1889, the year that the spires of St. Patrick's were completed—obviously in competition with it. It is all the more surprising that for some reason he does not mention Notre-Dame (*La Paroisse*) in Montreal, the obvious predecessor of St. Patrick's—not in form (too early, too "Gothick"), but in social function—and an aggressive statement of denominational supremacy in a non-Catholic environment. One would assume the omission deliberate (no Canadian examples) except that Frank Wills's Cathedral in Fredericton, New Brunswick is mentioned; nor can the explanation be lack of documentation, since *La Paroisse* and its distinctive sort of architectural statement have been the subject of considerable scholarship, most notably Frank Toker's fine book, which won the Society of Architectural Historians award as the best for 1974.

But this is a minor cavil. The book is a splendid



contribution to knowledge in general and American architectural history in particular.

ALAN GOWANS  
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HARLAN HAGUE. *The Road to California: The Search for a Southern Overland Route, 1540-1848*. (American Trails Series, number 11.) Glendale, Calif.: Arthur H. Clark. 1978. Pp. 325. \$20.50.

The southern trail to California—a route that ran along the present United States–Mexican border into the Los Angeles basin—grew in importance as Americans pushed westward. Some chauvinistic writers would have us believe it was an American invention, but Harlan Hague provides a nicely balanced account of the opening and reopening of the southern route by Spaniards, Mexicans, and Americans.

Three of the eight chapters in *The Road to California* treat Spain's explorers. Beginning with Cabeza de Vaca and Coronado and ending with the late eighteenth century expeditions of Juan Bautista de Anza, Francisco Garcés, and Domínguez-Escalante, Hague takes Borderlands specialists along familiar trails. He also devotes much of a chapter to the lesser-known efforts of newly independent Mexico to reopen the road from Sonora to California. American trappers enter the scene at the halfway mark of this study. In many cases the Americans followed trails that Mexicans already knew. Hague follows his chapter on the mountain men with separate chapters on the 1846–47 military expeditions of Stephen Watts Kearny and Philip St. George Cooke. The opening of a wagon route to California by Cooke provides a logical end to the story.

In a concluding analytical chapter, Hague argues that the "Gila River route" is a modern misnomer for the southern trail, which followed the Gila only part of the way. In his conclusion and throughout the narrative, he reminds us that Indians frequently pointed pathfinders in the right direction and that the transfer of new geographical knowledge often did not occur because travel narratives became buried in archives.

Specialists will find little that is new in this book, originally a doctoral dissertation done at the University of Nevada, Reno, in 1974. Hague has based his work entirely on published accounts and the outline of the story he tells is well known. No one, however, has tied these different threads of exploration together in a single narrative volume. Other treatments of the southern route to California, such as Odie B. Faulk's *Destiny Road: The Gila Trail and the Opening of the Southwest* (1973) and Ferol Egan's *The El Dorado Trail* (1970), take off at the point where

Hague's study ends. Specialists and general readers, then, will welcome *The Road to California* as a convenient and well-told summary of the story of three hundred years of exploration. Two caveats: Hague's use of Spanish names is capricious and omissions in his notes and bibliography make this work an unreliable guide to sources.

DAVID J. WEBER  
Southern Methodist University

GAVIN WRIGHT. *The Political Economy of the Cotton South: Households, Markets, and Wealth in the Nineteenth Century*. New York: W. W. Norton. 1978. Pp. xv, 205. \$10.95.

If any historians still believe that econometric history (sometimes called "cliometrics") offers little except esoteric methods and bizzare, ahistorical conclusions, this splendid book by Gavin Wright should finally persuade them otherwise. Like other cliometricians, Wright values the explanatory powers of economic theory, but he wisely cautions that theory designed to explain the modern world cannot be mechanistically applied to the past. Five key assumptions in modern economics he deems "fundamentally inappropriate to antebellum America": that the quest for profit maximization explains behavior; that relative prices indicate relative scarcities of the factors of production; that production for the market and production for consumption at home are the same except for transportation costs; that relative increases in per capita income best measure relative economic progress; and that real or potential changes in income explain people's political behavior. When he replaces these assumptions with others more appropriate to the historical context of the antebellum South, Wright comes to conclusions that he too modestly claims "do not sound original," but he adds, justifiably, that it is valuable "to show that they can be derived from simple models of economic behavior and supported with econometric evidence" (p. 4).

His modified theory reveals two features of the economy that provide significant interpretive insights. The first is that wealth, rather than income, is the most important determinant of antebellum farmers' behavior. The planters' major form of wealth was slaves; that of the nonslaveholders was land. Labor scarcity during the antebellum years prevented nonslaveowning farmers from expanding output beyond what available family labor could produce. But those able to buy slaves could expand their production to the extent of their resources, something planters would do until (using economic terms) the price of slaves equaled their marginal



product. To protect their wealth in land, non-slaveowners adopted a "safety first" behavior (p. 70), producing for subsistence before attempting a marketable surplus, thereby stressing wealth maintenance over income maximization. Slaveowners, not dependent on family labor, increased their marketable output by increasing their slave force, thereby increasing both their income and their wealth in slaves. Plantations became large, therefore, not because large-scale production brought economies of scale but because labor as a commodity could be allocated to production on a level unavailable to farmers whose labor supply was limited.

The second feature revealed in Wright's analysis is that the prosperity of cotton producers before the Civil War and the distress of postbellum cotton producers were not functions of the relative efficiencies of production but rather of the changing demand for cotton. Thus, what appears to be the profitability of slavery is really the profitability of growing cotton in a time of strong demand, while the unprofitability of tenancy and sharecropping is really the unprofitability of growing cotton when demand was stagnating. In the antebellum years, differences in risks involved in wealth maintenance explain why planters responded to the strong demand while nonslaveholders did not.

Anything threatening to depress the value of slaves threatened to diminish the wealth and power of the planters. There were two such threats. Within the South, rising slave prices (reflecting profits from slave labor) increased planter wealth but simultaneously undermined support for the institution, potentially endangering the planters' wealth, because rising slave prices made it increasingly difficult for nonslaveholders to move into the slaveowner class. Matching this internal danger was another from outside—a change in attitude toward slavery that, if it persisted, would surely weaken confidence in slavery, depress prices, and diminish planter wealth. The planters' answer to these threats, Wright concludes, was an attempt at "peaceful secession" (p. 147), which they mistakenly thought possible.

Wright's announcement that his findings tend to support the traditional view of slavery is, of course, wrong, simply because there is no traditional view. What he means is that his findings contradict those of other cliometricians. His accomplishment is to show that the formidable tools of economics, when imaginatively adapted to the historical context, yield important historical insights. This makes his book invaluable for both its interpretations and its methods.

HAROLD D. WOODMAN  
*Purdue University*

WILLIAM M. WIECEK. *The Sources of Antislavery Constitutionalism in America, 1760–1848*. Ithaca, N.Y.: Cornell University Press. 1977. Pp. 306. \$12.50.

Building on the work of Aileen Kraditor and Robert Cover and saturating himself in the primary sources, William M. Wiecek has produced a masterful analytical survey of the constitutional arguments over slavery.

Wiecek begins with what he considers the most significant source of antislavery constitutionalism, the Somerset case. In 1772, the highest common-law court in England handed down a decision stating that "contract for slave of a slave is good here" but also that slavery is so "odious" and of "such a nature, that it is incapable of being introduced on any reasons, moral or political; but only [by] positive law" (pp. 30, 32).

American abolitionists interpreted Somerset to mean that slavery was incompatible with natural law. From 1772 until slavery was abolished in 1865, abolitionists, as Wiecek indefatigably demonstrates, made chronic reference to the implied principles of this watershed case. At least until the 1830s American antislavery people argued within the framework of what Wiecek calls the "Federal Consensus" that only the states could abolish or in any way regulate slavery in their jurisdictions. This consensus was already evident in 1776 when no one proposed that the new national government have any authority over slavery in the states. It remained secure under the Articles of Confederation, continued to be accepted by large numbers of abolitionists through the 1830s and 1840s, and was the basis of the original proposed "unrepealable" Thirteenth Amendment in 1860–61, guaranteeing slavery in the states where it was then extant.

In the Missouri Crisis, 1819–21, some divergence of interpretation of the federal consensus emerged. Proslavery forces argued that the federal government was required to take a benevolently neutral position on slavery expansion, while antislavery forces argued, on neo-Somerset grounds, for "containment."

Wiecek maintains, however, that the real breakdown in the consensus came when the abolitionist movement itself split into moderate constitutional, radical constitutional, and Garrisonian wings. The moderates accepted the restrictions on federal power vis-à-vis the states, but nowhere else. The radicals on the other hand were the architects of "antislavery due process." Building on the Fifth Amendment's deprivation of liberty and property clause, the privilege and immunities clause, and the guarantee clause and arguing from natural and common law perspectives, radicals interpreted the

Constitution and thereby the federal government's role, *everywhere*, as antislavery. The Garrisonians, emphasizing the fugitive slave clause, the three-fifths clause, the insurrection and domestic violence clauses, and the intentions of the framers (reinforced by the publication of "The Madison Papers" in 1840), held the Constitution to be hopelessly proslavery.

Wiecek clearly favors the radical constitutionalists' idea of a "living law"—people (not judicial elites) reinterpreting the Constitution to make it mean what they want it to mean. The Garrisonians, whom Wiecek describes succinctly, brilliantly, and sympathetically, fell into the error of "making the construction of the Constitution . . . the same as the Constitution itself" (p. 243). Their position turned out to be *de facto* conservatism, and worse. Proslavery politicians from Alabama and South Carolina could circulate Garrisonian pamphlets to advantage.

The book is an extraordinarily clear explanation of constitutional antislavery doctrine. Wiecek's demonstration, however, that abolitionists *used* neo-Somerset principles and the like, seventy years after the fact, is not enough to establish a causal nexus—especially since he provides precious little contextual analysis. In fact, because Wiecek does not write about new and changed historical conditions and circumstances and because he stops in 1848, we are left with the false impression of a chain of causality, from Somerset, *sui generis*, to the Thirteenth Amendment.

Wiecek also takes some untested and, in some cases, indefensible historical positions, that is, "The slavery controversy was the most important single influence on American constitutional development before the Civil War" (p. 15); and the Thirteenth and Fourteenth Amendments were somehow, in white supremacist America, people's acts (as distinct from *Dred Scott*), and thus part of the process of making the constitution "Everyman's Constitution." Still I can not think of a better place to turn for an analysis of the variety of constitutional positions on slavery in America than this valuable book.

GERALD SORIN

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PEYTON MCCRARY. *Abraham Lincoln and Reconstruction: The Louisiana Experiment*. Princeton: Princeton University Press. 1978. Pp. xviii, 423. \$25.00.

This book, winner of the L. Kemper Williams Award for the best work in Louisiana history in 1978, deserves much praise. It represents a prodigious amount of research, not only in the usual

sources but also in manuscript collections of the Library of Congress and other depositories. Many of the collections cited from Northern archives have been used little, if at all, by previous students of Reconstruction in Louisiana. The author has arrived at a new interpretation of some phases of early Louisiana Reconstruction, especially of Abraham Lincoln's role, and some of these interpretations will not be acceptable to many other scholars who have worked in the same area.

Peyton McCrary gives a picture of antebellum Louisiana that differs little from the traditional and includes an account of the elections of 1860. He then turns to the Butler period, and, like a number of other recent scholars, he gives the much-hated Massachusetts politician high marks as an occupation commander. With Nathaniel P. Banks, who was in command in New Orleans for almost all the remaining time covered by the book, the author has little patience. He sees Banks as thwarting Lincoln's plans for Louisiana by favoring the "moderate" Unionists, led by Michael Hahn and James Madison Wells, over the "Radicals," who included such white men as Thomas Jefferson Durant and William R. Crane and free men of color like Francis Dumas and Jean Baptiste Roudanez. Then he describes a counterrevolution after Lincoln's death in which Andrew Johnson favored conservative Unionists and not-very-repentant ex-Confederates over Radicals, white and black.

Like a number of other recent writers on Louisiana Reconstruction, McCrary sees that the attempt at Reconstruction failed, deplores this failure, and looks for some way an effort to build a new society could have succeeded. The "revolution" did not succeed, and revolutions, being good, should succeed. The obvious conclusion, in the case of Louisiana, is that the revolution should have been put in the hands of Radicals, black and white, and then it might have succeeded.

In pursuing his thesis, McCrary reaches some apparently original conclusions. He rejects the contention that the Louisiana state constitutional convention of 1864 was oriented toward the artisan class and maintains that it was "bourgeois" in composition and action. More importantly, he concludes that Abraham Lincoln, just before his death made a conscious decision to move to the left and align himself with the Congressional Radicals' Reconstruction policy. So far as this reviewer is aware, this contradicts the conclusion of all other reputable scholars who have studied Lincoln's attitudes. Many, indeed, believe that Lincoln, in his last speech only two days before his death, was signaling a move away from, not toward, the Radical Republicans. McCrary presents no new evidence to

justify his contrary conclusion; in fact, it could be termed an assertion rather than a conclusion.

Nonetheless, in most respects this is an excellent book. The appendixes are useful, the bibliographical essay is excellent, and the well-written text is crammed with information. McCrary's contribution to the literature of Reconstruction may be somewhat controversial, but it is also significant.

JOE GRAY TAYLOR  
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DAVID A. NICHOLS. *Lincoln and the Indians: Civil War Policy and Politics*. Columbia: University of Missouri Press. 1978. Pp. vii, 223. \$16.00.

This book has three major areas of concentration. The first involves the problems caused by Confederate control of the Indian Territory and the response of the Lincoln administration to the pressures that emanated from that situation. Principal among these problems was the presence in Kansas of southern Indians who remained loyal to the Union. The Cherokee followers of John Ross are given much attention as is Jim Lane who represented Kansans in their effort to have the refugees returned to the Indian Territory.

The second area of focus is the Santee Sioux uprising of 1862 in Minnesota and its repercussions. The situation in Minnesota, especially in regard to the execution of Sioux prisoners, best illustrates how Lincoln's political acumen was put to a severe test given the exigencies of the war to save the Union. After considering the atmosphere of hate in which the military trials were conducted by General Henry H. Sibley and the extraordinary humanitarian effort of Episcopal Bishop Henry B. Whipple to save the condemned Sioux, David A. Nichols gives the following judgment of Abraham Lincoln: "Without him, all of the three hundred three probably would have been executed. On the other hand Lincoln still ordered the largest official mass execution in American history in which the guilt of the executed cannot be positively determined. He also acquiesced in concessions to Minnesotans that resulted in further injustice to the Indians. Nevertheless, . . . Lincoln's actions were relatively humanitarian" (p. 117).

The third major topic is the efforts of reformers to influence the Lincoln administration and the policies that were adopted in dealing with the native Americans. Nichols argues that, while Lincoln made a qualified pledge to reform the Indian system, he practiced political patronage in making appointments to the Indian service and in other ways contributed to the fraudulent administration of Indian affairs. He also gave a general endorsement to

the principles of reform, but at the same time used the military to concentrate Indians on reservations, thus making way for white expansion across the Great Plains. Nichols takes issue with Richard Ellis's *General Pope and U. S. Indian Policy* (1970), arguing that the military was also infected by political ambition. Thus, "Pope's proposals sapped the lifeblood of the reform movement. They were the old policy in new, militaristic dress, with the trappings of reform and not the substance" (p. 153). The use of the military to implement a policy of concentration is offered as evidence that Commissioner of Indian Affairs William P. Dole was more in agreement with white expansionists who saw Indians as an inferior and dying race than with reformers who favored assimilation.

While this is undoubtedly the best book published on Indian affairs in the years of Lincoln's presidency and while it lends itself admirably to an understanding of the "Peace Policy" of Grant's administration, there seems to be a missing element in its discussion of reform. Nichols does not appear to recognize that assimilation was the only viable goal of reformers, given the ethnocentric character of American society in the nineteenth century. He employs the term "racist" freely (even loosely) and compares white attitudes toward Indians and those toward blacks. But it was, in fact, the ethnocentrism of white Americans that determined the character of Indian reform. Realistic reformers accepted expansionism with the resulting envelopment of Indian lands and looked for a solution to the problem of Indian-white relations within that context. The goal of the "Peace Policy" and of the Dawes Act was to prepare Indians in a short time for assimilation into the "superior" culture as their reservations came under pressure and were eventually opened to white occupation. What separated nonreformers from reformers was whether they considered Indians to be biologically as well as culturally inferior. If native Americans were only inferior in a cultural sense, they were, according to the reasoning of reformers, capable of making the transition from barbarism to a dynamic and fast-growing civilization.

HENRY E. FRITZ  
St. Olaf College

FRANCIS PAUL PRUCHA. *American Indian Policy in Crisis: Christian Reformers and the Indian, 1865-1900*. Norman: University of Oklahoma Press. 1976. Pp. xii, 456. \$19.50.

General Ulysses S. Grant assumed the presidency in a time of peace that was perennially punctuated by war between the United States and the Plains Indians. To address the Indian question, Grant ap-

pointed a board of commissioners, modeled on the U.S. Christian Commission, an organization that had provided Union troops with church services and religious literature. Prominent Protestant laymen, mainly businessmen with an interest in humanitarian causes and the proper training of youth, comprised the majority of the commission. In their first report, they attributed Indian militancy to treatment "unjust and iniquitous beyond the power of words to express" (p. 38).

The commissioners enjoyed an influential voice in government policy for the remainder of the nineteenth century, and Grant's own "Peace Policy" featured the employment of other Christian critics as agents to the warring tribes. He even hoped the Quakers might make the warriors pacifists; more frequently, the tribes converted their agents to militancy. Francis Paul Prucha's analysis of the role of Protestant reformers in the revision of Indian policy explains fully and elegantly why Grant and his successors so enthusiastically coopted their severest critics.

Reformers believed that rights guaranteed Indians by laws and treaties required protection and that the government should bend its efforts to make Indians fit to enjoy not only the rights they already had but others they might gain by full citizenship. They believed that warriors could prepare for citizenship by living on segregated reservations of perpetually diminishing size, studying English and the agricultural and mechanical arts, and farming individual properties. Though some Protestants argued that Indians should enjoy "religious liberty," they saw nothing in aboriginal culture worth preserving and enthusiastically abetted the disintegration of tribal possessions, government, society, and religion, in the name of individualism, progress, and uniform "Americanism."

After 1880, new organizations, notably the Indian Rights Association, the Women's National Indian Association, and the Lake Mohonk Conference, did their homework carefully, lobbied systematically, informed the public zealously, and provided the government with several commissioners of Indian affairs. They saw to it that Indians received Christian missionaries, moral and vocational education, private property, and civil service reform, the better to homogenize them with the rest of the population.

The mainline reformers also did their best to exclude Catholic and Mormon influence from the Christianizing business. Faced with expanding Catholic interest in Indian education, they persuaded Congress to abandon the practice, which originated in 1819, of subsidizing Indian schools directed by religious societies.

The reformers often compared Indians with im-

migrants, whose peculiarities and political influence made them anxious. They were sure that both peculiar peoples could be assimilated. Lacking respect for or intimate knowledge of the societies they sought to transform, the reformers failed to make the Indians into one-hundred-percent Americans. They succeeded well enough, in concert with less disinterested advocates of land allotment and tribal disintegration, in reducing the power, property, and cultural cohesion of formerly independent and self-sufficient Indian nations.

Prucha's portrayal of the reformers' ruthless efforts exhibits in sharp focus and fine detail how the "search for order" in American society might define and direct a "deviant" and disorderly people. The author offers the clear and systematic account of policy we have come to expect from his earlier work on nineteenth-century Indian affairs. Although several of his chapters discuss events already well documented by other scholars, his contextual definition and rigorous exemplification of what "assimilation" meant to Protestant reformers of the late nineteenth century represents a unique and valuable contribution to our cultural history.

MARY YOUNG  
*University of Rochester*

DANIEL F. LITTLEFIELD, JR. *The Cherokee Freedmen: From Emancipation to American Citizenship*. (Contributions in Afro-American and African Studies, number 40.) Westport, Conn.: Greenwood Press. 1978. Pp. xii, 281. \$18.95.

With *The Cherokee Freedmen*, Daniel F. Littlefield, Jr., has made a notable contribution to both black and Indian history. The book analyzes the freedmen's struggle to establish their rights to citizenship in the Cherokee nation and consequently their rights to land and tribal funds. And it is Littlefield's thesis that the freedmen's struggle, one heartily resisted by Indian leadership, "helped lay the groundwork for inroads upon Cherokee autonomy over internal affairs of the tribe and . . . contributed ultimately to the dissolution of the Cherokee Nation" (p. xi).

The Cherokees began holding slaves in colonial times, took slaves with them to Indian territory, and on the eve of the Civil War had approximately twenty-five hundred bondsmen, who were not freed until June of 1863 by an act of the Cherokee National Council. The Cherokees then allowed the freedmen to live in their territory but did not consider them a part of the tribe. By the Treaty of 1866, however, blacks who resided in Cherokee country or who would return there within six months were supposed to be adopted into the tribe and were given claims to land. The Cherokees im-



mediately adopted blacks according to the treaty stipulations but then spent the next forty years trying to prevent the treaty's enforcement. Particularly thorny was the issue of the six months' limitation clause, which in effect seemed to deny citizenship rights to many blacks who had been a part of the antebellum nation but who because of hardships had not come back to Indian territory within the limited time allowed. For forty years United States officials tried to dictate policy regarding the rights of freedmen, and for forty years the Cherokee resisted.

Ultimately, the former slaves won a significant victory—with many being allowed to participate in the final allotment program. As Littlefield points out, however, the victory was jaded at best. The struggle pitted the Cherokees against the federal government and “was one of the several major influences that had weakened the Cherokee nation and had ultimately led to its dissolution” (p. 256). Moreover, the freedmen who were citizens of the Cherokee nation had been tolerated and had been given tax-free use of the land. But with allotments and the destruction of the Cherokee nation, freedmen were pushed into an Anglo-dominated society at a time when racial hatred was reaching an apex.

Littlefield's volume was excellently researched and written. It has no major stylistic nor mechanical flaws. Some readers might wish, however, that the author had devoted more space to the sociocultural aspects of the freedmen's life among the Cherokees. Only one of ten chapters deals with those aspects. Perhaps a more descriptive title would have warned those who expected an in-depth treatment of the development of the freedmen's community. These suggestions notwithstanding, Littlefield has produced a valuable book that should be examined by scholars interested in Indian history, black history, or the development of race relations.

JAMES SMALLWOOD  
Oklahoma State University

BEN MERCHANT VORPAHL. *Frederick Remington and the West: With the Eye of the Mind*. Austin: University of Texas Press. 1978. Pp. xviii, 294, \$15.95.

Frederic Remington and the artistic and literary impressions that he recorded of the American West are subjects that have long intrigued students of this region. This present work seeks to transcend the parameters of traditional biography in order to chronicle the intellectual and creative odyssey that led Remington to portray the West in his own particular fashion. His fascination with the transient and elusive nature of the West was manifest in his illustrations, paintings, bronzes, and literary works.

As with other artists who traveled in the West in the latter half of the nineteenth century, Remington sensed the ephemeral nature of this often violent and always vigorous frontier. It was the men of the West caught in an intense struggle with the Indians, animals, and elements of life in that region that captured Remington's artistic attention. He was not given to portraying the landscapes of the West nor to using women as fundamental subjects in his work.

Remington's western sketches were initially rejected by eastern journals which preferred more “genteel” subjects or saccharin allegorical themes, but the purchase of his initial western portfolio by the *Outing Magazine* and the public acceptance of his work in that publication led to contracts with *Harpers* and *The Century*. His career as a western illustrator had begun. His illustrations were soon complemented by essays that he authored on the West. Mildly iconoclastic, these essays challenged, as did his illustrations, the pervasive eastern view of the lands beyond the Mississippi River.

Familiar with the techniques of art and literary criticism and the fundamental themes of the intellectual and social history of the nineteenth century, Ben Merchant Vorpahl provides the most sustained analysis of the motive spirit behind Remington's work yet to appear. At times this searching for Remington's artistic motivations may be overstated. It is difficult to accept that in 1864, as Remington approached his third birthday in Canton, New York, he felt “the war as imaginative energy . . .” (p. 12). It is true that a substantial portion of his work focused upon martial themes, for more than seven hundred of his pictures utilized war or conflict as explicit themes. And this would, as Vorpahl notes, suggest “a strong impulse towards destruction” (p. 11), but it might also reflect a mirroring of the events that transpired on the plains in the last stages of the Plains Indian Wars. Yet, one cannot deny that he was fascinated by war and warriors, be it on the plains of the West, the streets of Chicago, or the sands of Cuba.

Attractively printed, this volume is a perceptive analysis of Frederick Remington and his art and provides an exemplar for the examination of other western artists.

PHILLIP DRENNON THOMAS  
Wichita State University

DANIEL J. WALKOWITZ. *Worker City, Company Town: Iron and Cotton-Worker Protest in Troy and Cohoes, New York, 1855-84*. (The Working Class in American History.) Urbana: University of Illinois Press. 1978. Pp. xv, 292. \$14.00.



From 1855 to 1884 the neighboring Hudson River cities of Troy and Cohoes, New York, ranked among the most important industrial centers in the United States. Troy boasted several major businesses including iron mills, foundries, and the shirt, collar, and laundry industry. Cohoes remained dominated by a single firm that, for a time, owned the largest cotton mill in the world. Despite these differences, both communities endured similar periods of violent strife. In *Worker City, Company Town*, Daniel J. Walkowitz examines this labor protest within the context of a maturing capitalist economy. He tests the theory advanced by Edward Shorter and Charles Tilly, based on their studies of France, that strikes are more likely to occur in a poly-industrial city than in a one-company town and then only after immigrant workers have completed the process of adaptation to their new environment.

Troy exemplified the worker city. Its chief labor organization, that of the Iron Molders, stood as one of the strongest and most militant unions in America. A thick network of social, political, and religious organizations produced a cohesive subculture within the predominantly Irish worker population. When management tried to crack down on labor in the 1870s, workers reacted with equal vehemence; Troy suffered such intense violence that some described it as a "reign of terror" (p. 6). At the end Troy's unionists, strongly supported by upwardly mobile Irish working-class representatives in City Hall and on the police force, retained both their high wage rates and control over terms of employment.

Several factors, according to Walkowitz, inhibited the growth of a labor movement in Cohoes. Permanent organization proved difficult since so many of the city's young women millworkers, who comprised the bulk of the labor force, expected to leave their employment soon after adolescence. Company paternalism, which influenced every facet of the workers' lives, hindered growth of an independent subculture. Newly arrived French Canadian immigrants, however, with their distinctive language and customs, helped to break this domination. When the operatives finally rebelled, they won some important concessions although their success never equalled that of their neighbors.

Walkowitz's study generally confirms the Shorter and Tilly findings. He notes, however, that unlike the movement in Europe a "radical vision of a fundamentally reorganized society did not motivate labor" (p. 252). America provided sufficient mobility and democracy, as plainly demonstrated in Troy, to transform any sociopolitical visions into "pragmatic economism: pure-and-simplism" (p. 253). Yet labor had won a Pyrrhic victory. With wages some-

times 20 to 30 percent higher in Troy and Cohoes, employers in search of lower costs gradually moved to other regions. Both cities, the author states, became industrial wastelands.

One of the best features of Walkowitz's approach is that it is not confined to a study of trade unionism but also stresses worker culture and values. At times he needlessly repeats his central thesis and sometimes excessive detail clogs the narrative. On the whole, however, Walkowitz's book makes a valuable contribution to labor and social history.

GRAHAM ADAMS, JR.  
Mount Allison University

RICHARD H. TIMBERLAKE, JR. *The Origins of Central Banking in the United States*. Cambridge: Harvard University Press. 1978. Pp. ix, 272. \$18.50.

In this book Richard H. Timberlake, Jr., sets out to trace the origins of central banking in the United States. At this task I believe he fails. Despite this, Timberlake's book is a useful and interesting discussion of certain aspects of the monetary history of the United States.

Most of the book is concerned with the currency. Of the fourteen chapters, eight have been published elsewhere in whole or in part. Taken together these chapters provide a cogent analysis of the history of currency in the United States in the nineteenth century. Five of the six new chapters concern central banking or monetary control, but their inclusion does not turn the book as a whole into a discussion of the evolution of central banking theory in the United States. The last chapter seems gratuitous: a discussion of the monetary and banking theory of Henry Thornton and Walter Bagehot and certain episodes in American monetary history with little effort at linking them together.

Combination of currency discussions and central bank theory is difficult because, as time passed, two topics that formerly had much in common grew apart. By 1873 there was little relationship between the debate over the currency and the development of central banking. The former, which principally concerned the standard, continued through the 1890s, ending finally with the Gold Standard Act in 1900. The development of central banking theory began to diverge from the currency discussion shortly after the Civil War, eventually culminating in the Federal Reserve Act of 1913. The divergence is shown by the fact that the Federal Reserve Act marked the ascendance of a theory of banking, the real bills doctrine, that was incompatible with the ideas of both sides of the currency debate, even though the Federal Reserve Act explicitly reaffirmed the gold standard. Central banking dis-

cussions had shifted away from currency to the larger questions of the total money supply and the volume of credit.

Timberlake does not recognize this divergence and, as a result, this book on central banking theory never mentions bankers; it is as if all contributions after 1836 to the development of central banking doctrine were made by politicians or government bureaucrats. Nowhere are bankers, individually or as a group, mentioned as a part of the process of evolution. There is a passing mention of the New York Clearing House's issue of certificates and the Indianapolis Monetary Convention but no mention of the National Citizens' League or the role of bankers in the National Monetary Commission. The role of academics is treated only somewhat less perfunctorily.

If this book is judged by how well it treats the development of central banking, it will be found wanting. If what one desires is a penetrating analysis of currency debates during the nineteenth century, particularly with regard to the antebellum period, the book is much more successful. This issue and period deserve more attention than they have received in the past. For these reasons Timberlake's book is worth the attention of scholars interested in monetary history before 1900.

ROBERT CRAIG WEST  
*Drake University*

JAMES D. NORRIS. *R. G. Dun & Co., 1841-1900: The Development of Credit-Reporting in the Nineteenth Century.* (Contributions in Economics and Economic History, number 20.) Westport, Conn.: Greenwood Press. 1978. Pp. xx, 206. \$17.50.

Creditworthiness was crucial to the colonial businessman. His standing in the commercial world depended on his "respectability," meaning the virtues of honesty, prudence, reliability, integrity. He was someone to be trusted, whose word was his bond and who would go extraordinary lengths to protect his reputation. This informal, though rigorous, rating system became outmoded as American business expanded, became impersonal and institutionalized, and, above all, involved paper rather than face-to-face transactions. This transformation created a need for a systematic way of gathering and reporting assessments of the management and prospects of firms with whom one did business. Such information had a value in the marketplace, and R. G. Dun & Co. was one of the earliest and most successful of the firms to provide such a service.

James D. Norris has written a clear, informative, and useful account of the formation, development, and practices of Dun's credit-rating firm. Though

some of the associates carried the title of partner, they were really employees and invested business skills rather than capital. This created personnel problems, and more than one disgruntled employee left to start a rival agency. There were legal problems as well. For many years Dun dared not publish the data on which his ratings were based for fear that he would be exposed to libel suits. Clients could obtain such information only by consulting the records at the agency, where they could read but not copy the reports. Very cumbersome and difficult. Eventually the courts shielded credit-rating firms from damage claims, and Dun developed a reporting system that could not possibly give offense. Essentially, ratings were based on overall financial strength: the stronger the firm, the higher the rating. This favored the larger firm over its smaller rivals, no matter how poorly managed, and intensified the trend toward bigness in business.

Although this is not a personal biography, Norris perhaps pays too little attention to Dun's private affairs. A heavy drinker, a lavish spender, and an investor with an almost unerring proclivity for high-risk, unsound properties, it is a wonder that his firm remained untarnished, the more so since in the last years one of his associates was plundering the agency. Given Dun's unimpressive record in his personal life, as private investor, and as manager-supervisor, just how accurate were his business ratings? We shall never know; but how many firms went under which should not have, and how many firms survived only because Dun mistakenly favored them? If the ability to borrow is as crucial as this reviewer believes, then Dun assumed a much larger responsibility than he could possibly have known. But then he was not really concerned with such matters. His interest seems to have been making money—and that he did.

PETER J. COLEMAN  
*University of Illinois,  
Chicago Circle*

THOMAS R. NAVIN. *Copper Mining and Management.* Tucson: University of Arizona Press. 1978. Pp. xxi, 426. Cloth \$16.50, paper \$9.75.

This volume is the product of a professor of business administration at the University of Arizona who has worked extensively in the area of mining management and mining corporation strategy. Hence, Thomas R. Navin is well qualified to deal authoritatively with his subject. Despite its substantial length, the work is an introductory survey intended to serve as a handbook for the executive trainee or as a sourcebook for classes. While not history per se, it employs the historical case-study method throughout the thumbnail sketches of twenty-five of the world's largest copper mining companies. Wo-

ven into the cases are accounts of the development of the Western world's major copper deposits. The author's clarity of presentation is very useful in giving meaningful insights to a field whose bibliography is otherwise based chiefly upon geological, technical, or romantic treatments.

While sympathetic to his corporate subjects, Navin is not adulatory. His criticisms center on entrepreneurial blunders and inefficiencies, and he concludes that the chief offenders are institutional prejudice and inertia, provincialism, ultraconservatism, unwillingness to innovate at risk, and, of course, the universal managerial vice of falling into the assumption that paperwork is an end in itself. The reader concludes that the handwriting is on the wall of a once-virile corporation when three symptoms appear: the replacement of "production men" by finance-oriented executives, cost-cutting in exploration and innovative technology research, and, finally, a move to the blighting atmosphere of New York City (or, a century ago, to Boston). Apparently a drafty construction-site shack in Nevada is more conducive to original thought than is the well-upholstered View from the Top.

From the standpoint of this reviewer's personal interests, the book serves to correct a number of time-honored suppositions, one of which is the frequent assertion that the inception of Western copper mining in the 1880s came in response to the electrical industry's growing demand for wirebar. Navin shows that this cannot be so: the electrical industry's demand did not become significant until the late 1890s. Instead, Western copper production grew out of the hope of reviving moribund silver mines by hoisting their copper values as a cheap by-product. And I noted with deep interest that the old copper kings and silver nabobs apparently used sound strategy in buying mineral prospects from "desert rat" prospectors, for the author suggests that there is somehow an inverse relationship between a company's operational success and its success in exploring.

Unfortunately, the book lacks a systematic bibliography, and annotation is confined solely to exegesis. There are a few small but jarring errors of fact, of which a typical example is the assertion that the Calumet and Hecla "was not blessed with native copper" (p. 12). Perhaps the author confuses the C and H conglomerate ore with the old "mass" copper, but native metal was virtually all that the Keweenaw copper range produced. These lapses shade into insignificance, however, when compared to the overall value of this study. I strongly recommend it to economic historians, students of management, those who need a reference work, and all other interested parties.

OTIS E. YOUNG, JR.  
Arizona State University

DAVID M. KATZMAN. *Seven Days a Week: Women and Domestic Service in Industrializing America*. New York: Oxford University Press. 1978. Pp. xviii, 374. \$14.95.

At the high tide of American industrialization, a woman worker was more likely to be a domestic servant than an industrial laborer. Despite this fact, historians concerned with women's work have looked only superficially at this occupation and the women who entered it. Servants have been invisible to American historians in much the same way they were unobtrusive in well-ordered households. For historians, as for modern American society generally, work done in the home has been outside the purview of "real" work.

David Katzman's *Seven Days a Week* deals with women domestics in the years between the Civil War and World War I. It is the first vigorous, detailed study of American servants' lives and work and contains a wealth of fascinating material about immigrant and black domestics' living and working conditions, American attitudes toward servants, and servants' attitudes toward their work. Although the book generalizes about servants as a group, the author is always sensitive to ethnic, regional, and racial differences: experiences of black servants in the South, for example, were quite different from those of immigrant women in the North. Katzman also analyzes work as a set of social relationships between servants and mistresses. Unfortunately, this part of the study is not altogether successful. The author's argument that Northern mistresses often sought to gratify their own emotional needs through their relationships with servants falls flat for lack of evidence.

Katzman's major conclusion is that, although servants' wages and conditions were generally equal to or better than those of female industrial operatives, domestic service was looked down upon by working-class women generally and by many servants themselves. As a general statement, this conclusion can be questioned. Lawrence Glasco's study of immigrant life cycles in Buffalo, New York, for example, indicates that for most Irish and German girls domestic service was a kind of apprenticeship, an inevitable and desirable transition period between childhood and marriage. Nevertheless, Katzman has marshaled abundant evidence to show that servants were frequently dissatisfied with their lot. Their complaints to social investigators, their rapid job turnover, and the rapidity with which women left service when other opportunities arose support his argument. Katzman also argues convincingly that the widening of female employment opportunities rather than the mechanization of housework depleted the servant population. Servants who had alternatives left the field to women who had the fewest options and the least power in

American society. The author does a good job of documenting the shift from immigrant to black servants as white women found more desirable industrial and white-collar positions and as blacks moved to Northern cities.

There are a number of weaknesses in *Seven Days a Week*. Katzman shies away from analyzing some of his most important findings. Too often he ascribes causation to the collective catchall of "industrialization, urbanization, and modernization" without attempting to explain changes in servants' situation in more precise terms. Nor does he tackle the questions of why domestic work became devalued in an industrial society and how that devaluation was linked to changing conceptions of women's roles and the privatization of the household. Nevertheless, Katzman's book is a pioneering study, of value to all students of work in industrial society.

NANCY SCHROM DYE  
University of Kentucky

SHEILA M. ROTHMAN. *Woman's Proper Place: A History of Changing Ideals and Practices, 1870 to the Present*. New York: Basic Books. 1978. Pp. xiv, 322. \$12.50.

When her work took her into the midst of contemporary controversies over social policies designed to improve women's lot, Sheila Rothman was struck by the fact that at root these battles were over two issues: first, woman's proper place in the society, and, second, the presumed conflict between what was good for women and what was good for children, families, and society at large. Believing that the second issue was something new in American history—"Our predecessors found interdependence just where we perceive competition and conflict," she writes (p. 4)—she examined the history of these issues to find out where assumptions had changed.

Rothman's analytical framework is based on four periods, each characterized by a widely accepted view of what woman should be. Her labels for these characteristic ideals and the rough dates of their ascendancy are (1) virtuous womanhood (1870-1900), (2) educated mothers (1900-20), (3) wife-companion (the 1920s and the 1950s, with time out for the Depression and the war), and (4) woman-as-person (the 1960s and 1970s). Using these categories to organize her material, Rothman tries to show how these ideals shaped the manner in which activist women in each period, responding to social expectation, affected social policy. "Virtuous women" engaged in voluntary associations with social and philanthropic goals; "educated mothers" became the prime movers in social welfare legislation; "wives-companions" retreated from serious social

reform and adopted birth control; and "women-as-persons" lead the liberation movement of our own time.

The principal forces leading to change in the definition of women's place, in Rothman's view, were technological development, growth of higher education for women, and reactions of male professionals, especially medical doctors, to women and their needs. Early chapters contain a synthesis of much recent work in women's history as well as Rothman's own analysis of the consequences for women of such innovations as running water, apartment houses, refrigerator cars, department stores, and typewriters. She provides careful discussions of the health question, which bedeviled early women's colleges, of the growth and effectiveness of voluntary associations with particular reference to the settlement houses, of the part women played in initiating and supporting progressive legislation, of the rather appalling story of the medical profession's destruction of the Sheppard-Towner maternity and infancy program, and of the dismal outcome of the drive for federally supported day care. In a deeply felt chapter called "The Family as a Zero Sum Game," she analyses the divisive and potentially damaging effects upon society of the recent drive for personal fulfillment.

The periodization that forms the skeleton of the book is necessarily a bit schematic, and the concepts themselves—as the author recognizes—overlap. Some of them had been around for a long time by 1870: the idea of educated motherhood, for example, was the principal rationale for the rapid expansion of women's educational opportunities after the Revolution. One might argue that all four have coexisted through most of the twentieth century. It is also apparent that the particular ideals she has chosen as characteristic, insofar as they affected women's actual behavior, were chiefly operative in the middle classes. They did not fit well with many women's actual experience: can one imagine the steel workers' wives depicted in Margaret Byington's splendid *Homestead* finding it practical to make themselves into educated mothers in Rothman's sense? Or the working class wives in *Middletown* even attempting to become wives-companions?

The hazards in writing a synthetic study in a new field are considerable, but this book contains useful analysis of many particular developments of the past century. Rothman writes well and asks important questions, which will make the book useful to architects of social policy and students of social history.

ANNE FIROR SCOTT  
Duke University



DONALD L. WINTERS. *Farmers without Farms: Agricultural Tenancy in Nineteenth-Century Iowa*. (Contributions in American History, number 79.) Westport, Conn.: Greenwood Press. 1978. Pp. xvi, 145. \$15.95.

American historians traditionally have viewed farm tenancy as un-American. Not in the sense that it did not exist, but rather that it should not have existed. In a country with abundant land and a populace eager to own it, tenancy has seemed anomalous, a signal of institutional failure and human distress. Of course, someone had to be blamed, and historians identified the villains as large land speculators and "monopolists," rapacious moneylenders, and indeed the land disposition system itself, which allegedly facilitated the engrossment of princely estates and reduced the odds of acquisition by the toiling masses.

Donald L. Winters's *Farmers without Farms* establishes that, for nineteenth-century Iowa, the traditional view of farm tenancy is almost completely a myth. Winters considers all the key questions about tenancy as an economic institution: Why did some farmers rent rather than buy land? Did the farms of tenants and owners differ in size, crops grown, livestock raised, capital and labor employed, value per acre, and productivity? What determined the choice between share and fixed rents? How did landlords enforce contractual stipulations? To what extent was the agricultural ladder a reality? To answer these questions, he marshals an impressive body of data drawn from the manuscript census schedules, mortgage records, and tax lists; he examines carefully the terms of more than two thousand lease contracts registered with county recorders; and he makes good use of such traditional sources as agricultural journals, accounts of travelers, local histories, and published census reports. Most importantly, the data are examined systematically. Winters relies on economic theory to obtain testable hypotheses and interpret his findings. He performs many statistical tests, using linear regression and correlation techniques. Neither the theory nor the tests are obtrusive, however, and substantive issues occupy the forefront. Numerous tables make it easy to check the author's description and analysis.

In a short review, I cannot do justice to this short but meaty monograph. Its general thrust is that "the increase in farm renting, far from signaling economic malfunction, was a natural outgrowth of a normally operating market system. . . . For none was tenancy a barrier to landownership; nothing about it irrevocably committed farmers to a lifetime of renting. Indeed, for many it was a vehicle for gaining that coveted ownership. . . . Although they may have engaged in types of farming different from those of their landowner counterparts, renters,

whether share or cash, were as productive as owner-operators. Thus, tenancy did not undermine agricultural development and slow economic growth; in fact, by placing land in the hands of those who would put it to productive use, it probably enhanced both" (p. 107).

At places the author could have sharpened his analysis. Especially in the second chapter, tests for the statistical significance of differences between owners and tenants would make the analysis firmer. The data in table 2-6 provide a missed opportunity for an analysis of variance to distinguish between owner-tenant differences and intercounty differences. At a few points I object to the statement of the economic theory or to the way in which the regression analysis is carried out or reported. But these are quibbles. I doubt that the marginal improvements I can imagine would alter any of the major conclusions.

Overall, this is an excellent study, a model of how economic history should be done. As the author notes, its conclusions may not apply to other states. But it blazes a clear trail for those who will, one hopes, replicate the analysis for other areas. For Iowa, the conclusion is unambiguous: the traditional view of tenancy does not accord with the facts. Historians henceforth must accept farm tenancy as what it actually was, a useful and productive institution.

ROBERT HIGGS  
University of Washington

ROBERT G. ATHEARN. *In Search of Canaan: Black Migration to Kansas, 1879-80*. Lawrence: Regents Press of Kansas. 1978. Pp. xii, 338. \$14.00.

*In Search of Canaan*, the second major study of the black exodus to Kansas, appears only a year after *Exodusters* by Nell Irvin Painter. Since the books differ in organization and emphasis, it seems useful to compare their treatment of certain key issues—the causes of the exodus, the reaction of others to the migration, and the results for the exodusters.

Robert G. Athearn devotes less space than Painter to the causes of migration from the South. Athearn mentions several possible motivations: economic problems, disease, hope of advancement, promotional circulars, religious enthusiasm, and politics—both northern and southern—as well as the examples of earlier migrants and the influence of railroad and steamboat agents and confidence men. Yet he sees little difference between the black exodus and the general westward movement. His account is weakest in its failure fully to understand the relationship between white southern political violence and black economic subordination. Also



questionable is his assumption that southern black farm income often compared favorably to that of northern whites. Painter develops the question of causation more fully and convincingly.

The emphasis in this volume is on the reception accorded the exodusters in Missouri and Kansas and the reaction throughout the nation to the migration. Black migrants met a mixed response of aid and opposition in St. Louis and in Kansas. Republicans and reformers across the North assisted the exodusters for a combination of humanitarian and political reasons. Democrats and many businessmen, as well as some farmers and laborers who feared black competition, opposed the exodus because of economic and political concerns. Southern white reactions ranged from "good riddance" to physical restraint, offers of improved economic arrangements, and a search for immigrants as substitute laborers. Athearn is more detailed than Painter on this aspect of the migration, although some of his conclusions seem open to debate. Athearn believes many Kansas blacks opposed the exodus as much as whites, while Painter describes them as more helpful. White Kansans primarily opposed penniless black migrants according to Athearn, but Eugene Berwanger found strong support for black exclusion from Kansas even before the Civil War. Finally, Athearn concludes that a smaller group of migrants to Indiana influenced the election of 1880, a view not supported by other historians of the campaign.

*In Search of Canaan* offers more detailed information than *Exodusters* on who migrated and what happened to them in Kansas. Athearn differentiates between the better-equipped migrants from Tennessee and Kentucky in 1877 and 1878, who usually founded rural black colonies, and the millenarian blacks, whose sudden exodus in 1879 often deposited them as laborers in Kansas towns. Yet some questions remain. Quotations describing the exodusters as mostly old men, women, and children receive only limited support from apparently unused 1880 census statistics. The author also seems to ignore studies of other, more individualistic black frontiersmen when he attributes to migrants "a racial characteristic: gregariousness" (p. 256). Athearn concludes that some exodusters became successful, but many suffered from their move, in contrast to Painter's more optimistic judgment on the results of the migration. Perhaps neither author has written the last word on this aspect of the movement.

Athearn's research is sound, though Painter employed a wider range of manuscript sources. A colorful writing style enlivens Athearn's volume. Perhaps because Athearn has delved into a less familiar field for him, black history, his study does not seem to be as clear in focus and analysis as Painter's. Yet,

because of the differences between the two studies, each complements the other while making a contribution to the history of the exodus.

ALWYN BARR  
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JAY R. MANDLE. *The Roots of Black Poverty: The Southern Plantation Economy after The Civil War*. Durham, N.C.: Duke University Press. 1978. Pp. xvi, 144. \$8.75.

If, as Harold D. Woodman has argued recently in the *Journal of Southern History*, cliometricians have misapplied neoclassical economics to the post-bellum South, Jay R. Mandel has more than corrected their error. Mandel's thesis is direct and bold: much of the American South remained a plantation society until the 1950s, its protracted antebellum economy sustaining, in symbiosis, the roots of black poverty and Southern underdevelopment.

Building on his earlier analysis of Guyana as well as the ideas of Eugene Genovese, the author asserts that the New South, like the Old South, represented neither feudalism nor capitalism, but a unique mode of production that was at once modern in its market orientation and archaic in its social organization—its indispensable feature being a cheap and coercive labor system. Black slavery inevitably gave way to black tenantry, a system in which planter hegemony lived on and nonmarket forces determined wages and occupations. Plantation laborers were often free to move from unit to unit within the system, but rarely could they move outside the system. Deterred from land ownership, migration, or alternative employment and generally not needed or wanted outside the South, "black labor's bargaining strength and, therefore, income were severely limited while at the same time the plantation mode of production was rendered viable."

In turn, the agrarian economy was rendered stagnant. As long as Southern capital remained scarce and dear, while labor remained plentiful and cheap, the region would lag behind in the dialectic. In what might be styled as a labor theory of technology, Mandel links the low cost of plantation labor to the listless pace of Southern mechanization. Not until World War II and the second wave of black migration did the South experience sufficient structural incentive to shift from a labor intensive plantation economy to modern mechanized agriculture. And, since economic change is the handmaiden of political consciousness, it is no coincidence that the civil rights movement began just as the plantation economy ended.

From his hemispheric and Marxian perspective, Mandel presents this transformation of the Ameri-

can South as one of the significant chapters in New World history, part of a process in which one of many plantation societies entered a new stage, the working class leaving the "estates" and the planter class joining the bourgeoisie. This high ground of macro-history provides an arresting view of the roots of Southern distinctiveness, but when one descends to the underlying thesis—the roots of black poverty—the view is less satisfying.

If the stage of history that produced black poverty has passed, why does the pathology persist, especially in the North? For Mandle the "plantation legacy" explains the lag. As "late arrivals" in an advanced industrial economy, propertyless peasants could scarcely expect "occupational integration" in one generation. As for the role of racism, Mandle acknowledges that Northern racism reinforced the plantation economy, yet his analysis cannot comfortably account for racial discrimination outside the South. Racism necessarily accompanied the plantation economy, as the surviving superstructure of racial slavery, but the author doubts any "systemic" connection between racism and capitalism. Northern racism, therefore, might stand as an independent cultural force; similarly Jim Crow, obsessed with social control and most active off the plantation, may have assumed a career of his own. But Mandle's model cannot afford such flexibility. To concede racism an autonomous base of its own, from which it could explain black poverty without regard to the mode of production, would be to concede the thesis of the book.

For all its sophistication, the author's view of the New South appears single faceted and static. His interpretation of Booker T. Washington as "thoroughly southern and agricultural," a symbol of black subordination and plantation hegemony, summarizes the problem. Washington, the former slave and agrarian, was also thoroughly bourgeois. Like the New South at large, he was stretched between the pastoral and the industrial. In this same vein, Southern planters (some of them industrialists) maintained no monopoly on coercion and paternalism; Northern and New South industrialists (some of them planters) similarly smiled on seigniorial labor. The question remains, then, how persistent and pervasive was the plantation economy and can black poverty be explained without it? The origins and autonomy of racism vastly complicate the question.

WALTER B. WEARE  
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DONALD SPIVEY. *Schooling for the New Slavery: Black Industrial Education, 1868–1915*. (Contributions in Afro-American and African Studies, number 38.)

Westport, Conn.: Greenwood Press. 1978. Pp. xii, 162. \$14.95.

William Monroe Trotter, the militant editor of the Boston *Guardian* and one of Booker T. Washington's most persistent black critics, opposed the Tuskegee system of industrial education because, as he put it, "the idea lying back of it is the relegation of a race to serfdom." Donald Spivey, in the brief volume under review here, goes Trotter one better; industrial education was, in Spivey's words, "schooling for the new slavery." In a chapter entitled "Shine, Booker, Shine: The Black Overseer of Tuskegee," he likens Washington to a black driver under the slave system "who, given the position of authority over his fellow slaves, worked diligently to keep intact the very system under which they both were enslaved" (p. 66). When Washington established Tuskegee, he was only following the lead of his revered mentor at Hampton Institute, Samuel Chapman Armstrong, whom Spivey dismisses as a blue-nosed racist intent on preparing blacks for second-class citizenship. "The basic idea underlying the educational philosophy of Hampton Institute was the more efficient exploitation of [black] labor, to fitting the Negro in his place," he claims (p. 22). The same spirit prevailed on Washington's campus in Alabama, where "Booker T.'s educational practices were based on his desire to please whites and gain their support" (p. 63). The extension of the Tuskegee idea to Africa, described in the last and most original chapter of the book, was a malevolent enterprise from the start that was "given a big push from Americans interested in the stabilization of a world order based on white rule" (p. 111). Spivey describes the close ties between the Booker T. Washington Institute in Kakata, Liberia, and the Firestone rubber plantations nearby and concludes that the whole industrial education movement, from Hampton to Tuskegee to the shores of West Africa, was an attempt to solidify white rule through "a dissemination of educational ideas that were conducive to perpetual slavery" (p. 129).

This interpretation has, of course, had its defenders. Trotter and W. E. B. DuBois, among others, leveled similar charges at Washington and the Tuskegee system during the early years of the twentieth century, and their criticisms are echoed in much of the scholarly literature dealing with Washington's career. But the debate over Washington has moved to a much more sophisticated plane in recent years. August Meier, Louis R. Harlan, Harold Cruse, Samuel R. Spencer, Jr., Eugene D. Genovese, and most other commentators of the past two decades freely acknowledge Washington's limitations and his faults—his duplicity, his ruthless use of power, his advocacy of the Gospel of Wealth, and the se-

vere shortcomings of his educational ideas. But they also point out his commitment to black economic independence, note the all-black nature of the Tuskegee enterprise, cite Washington's behind-the-scenes efforts to oppose segregation and disfranchisement, and suggest that there is a good case for considering Washington a precursor of twentieth-century American black nationalism. Spivey will have none of this. He certainly has a right to his interpretation, but it does very little to advance the debate over the meaning of Washington's life and the consequences of his leadership. Nor is the author well served by the strident tone of his work. The conclusion on this poorly designed, exorbitantly priced book is inescapable: one must look elsewhere for an understanding of black industrial education and the man who was primarily responsible for its development.

CHARLES B. DEW  
Williams College

THOMAS SOWELL, editor. *American Ethnic Groups*. Washington: Urban Institute. 1978. Pp. viii, 249. \$7.50.

*American Ethnic Groups*, intended as an introductory study of "ethnicity in America and an exploration of the frontiers of knowledge in this field," is neither fish nor fowl. Several ethnic groups are discussed: blacks, Chinese, Japanese, Irish, Italians, and Jews. Most of the essays are general in nature, two are more technical and intended for specialists. The most satisfying essay is the editor's own, "Race and I.Q. Reconsidered," in which he moves away from the sterile heredity-environment argument on the subject to note that the I.Q. tests do have some validity as measurements for predicting "future performance in an academic setting, though [they] fail to measure innate potential." Thomas Sowell also points out how the I.Q. tests provide some clues for social and political policymakers and help identify talented youths from "unknown and nondescript high schools." Although nothing particularly new is offered, it is important to make some of these points clear for general readers.

The essays on the ethnic groups per se are somewhat less satisfactory but are nonetheless fairly accurate surveys. The cultural mores of the various groups are discussed and points like the importance of self-discipline for the Orientals and educational attainments for the Jews are noted. In each of the essays, however, there are statements, supposedly of fact, with which I disagree. For William Peterson to repeat Marcus Lee Hansen's oft-quoted observation that "what the son wishes to forget the grandson wishes to remember" as applicable to the Chinese and Japanese is merely passing on misinformation

uncritically. Hansen's remark has been widely accepted as gospel, yet I doubt whether careful analysis would substantiate its validity to any great extent. Alice Kessler Harris and Virginia Yans-McLaughlin, two highly regarded scholars, suggest some things in their otherwise excellent piece that are misleading. They attribute Jewish and Italian socioeconomic advancement at a rate faster than that of the Irish to the greater economic opportunities in the early twentieth century compared to those in the mid-nineteenth. I wish they had provided evidence to substantiate this point because my readings lead me to believe that economic opportunities were more abundant in the earlier period. Furthermore, they acknowledge fairly rapid German advancement in the very years that proved less fruitful for the Irish. The findings of some of the other essays in the collection lead to such conclusions as (1) ethnic group earnings vary according to individual educational attainment and local labor market conditions, (2) accomplished black academics have produced fewer works of scholarship but earn comparatively higher salaries than whites with equivalent academic records, and (3), interestingly, Oriental academics who have published more than white colleagues earn less.

All in all the collection is difficult to evaluate. It does present, generally, the findings of scholars in the 1970s, but several points are glossed over or mentioned merely because they support a given hypothesis. To whom the book can be recommended is another dilemma. Scholars certainly would gain little from reading it, yet the prose is not sufficiently stimulating for a general audience.

LEONARD DINNERSTEIN  
University of Arizona

EMORY LINQUIST. *Shepherd of an Immigrant People: The Story of Erland Carlsson*. (Augustana Historical Society, number 26.) Rock Island, Ill.: The Society. 1978. Pp. xi, 236. \$7.50.

Swedish Lutheranism in America found expression in only one body, the Augustana Lutheran Synod. But this unity did not emerge without difficulty; in the manner of other Scandinavian immigrants, the Swedes had to convert a state church tradition into a viable free church system in what some churchmen at the time viewed as an excessively liberal religious climate. The lines of contention were drawn between adherents to a high-church principle, generally supported by the Swedish-trained clergy, and those who favored a pietistic, low-church form of worship; included in the competition for the religious allegiance of the immigrants were such diverse groups as the Baptists, the Methodists, and the Episcopalians.

The dynamics and the process whereby Swedish-American Lutherans asserted themselves is ably presented in Emory Lindquist's well-researched biography of Erland Carlsson (1822-93). The ecclesiastical hierarchy in Sweden disapproved of emigration, which from the mid-1840s increasingly affected Swedish society, and interest in the religious fate of the emigrants therefore became the concern of a small group of liberal Lutheran leaders. The pioneer pastor was Lars Paul Esbjörg, who from 1849 worked among the scattered Swedish population in Illinois; his appeals for assistance brought Erland Carlsson to Chicago in 1853 to serve a Swedish Lutheran congregation.

Carlsson's career was undoubtedly distinguished. He was a man of great personal integrity, strength, and independence; by his own determination he rose from modest circumstances in Kronberg's *län* in the province of Småland to the respected vocation of a minister in the Church of Sweden. Early he encountered his bishop's displeasure for conducting devotional meetings outside the church, thus violating the conventicle ordinances; in America, he likewise faced challenges to his authority and mission, first from the well-known Swedish Episcopalian Gustaf Unonius, who sought official sanction for his efforts among the immigrants from the homeland, and later, in 1869, from the pietistic, anticlerical mission society within his own congregation.

His most enduring contributions were made in connection with the union of forty-nine Swedish and Norwegian congregations in 1860 to form the Augustana Lutheran Synod and in that same year with the founding of Augustana College and Theological Seminary; Carlsson played a prominent role in both these accomplishments, and in 1881-88 he succeeded the pioneer Minnesota pastor Eric Norelius as president of the synod. The emergence of these institutions had far-reaching implications for a distinct Swedish-American identity; in 1870 the Norwegians withdrew from the synod, leaving it a purely Swedish church body.

Swedish-American Lutheranism appears orderly and peaceful in its organizational growth when viewed in terms of the strife experienced among the Norwegian Lutherans, but, unlike the Norwegians, the Swedes never reached the strength of Norwegian-American Lutheranism in spite of an eventual numerical superiority. Lindquist does not address this issue, nor does he fully view Carlsson's mission in the broader context of American religious history. A more serious question may be raised in regard to Lindquist's ability to keep a proper scholarly distance from his subject; one rather suspects that Lindquist's steadfast loyalty to and admiration for Carlsson at times deprives the reader of knowing

the full scope of his humanity, and perhaps, on occasion, provides less than a fair hearing to Carlsson's opponents—his "sterling qualities" (p. 201) are never tarnished or even questioned.

ODDS LOVOLL  
*St. Olaf College*

EUGENE J. WATTS. *The Social Bases of City Politics: Atlanta, 1865-1903*. (Contributions in American History, number 73.) Westport, Conn.: Greenwood Press. 1978. Pp. x, 188. \$17.95.

The varied responses to the question of "who governs" have enabled political scientists and historians to chart the changing bases for policy and the role played by political leadership in society. Eugene J. Watts's study of post-Civil War Atlanta's politicians contributes more in the methodology he employs than in the results he secures.

First, Watts collected data on the social and political characteristics (eleven variables in all) of all candidates—eight hundred winners and losers. Second, Watts related these characteristics to the political and demographic context of Atlanta from 1865 to 1903, when basic changes in the political system occurred. Third, Watts compared each attribute with other attributes to determine the strength of certain combinations of characteristics. Finally, through multiple discriminant analysis, Watts examined the relative importance of each attribute to the outcome of elections. He presents the results in tabular form and explains them with clarity so that other scholars can readily replicate his procedures.

The results of this analysis are, generally, not surprising. The "social filter" of Atlanta politics ensured a group of candidates who were typically either businessmen or professionals, were financially secure, had a relatively lengthy identification with the city (though not necessarily natives or even southerners), and as the city expanded tended to reside toward the periphery. Though nearly one-fifth of the candidates were working-class representatives, they were nominated with the blessing of the economic elite. Except for property holding, none of the variables was a consistently significant predictor of victory or defeat, indicating that the nominating procedure filtered out the socially and politically inappropriate individuals. Finally, there was a remarkable stability in the social bases of Atlanta's politics throughout the period, despite the rapid turnover of officeholders. In this respect, Atlanta differs from Robert Dahl's New Haven and Robert Schulze's "Cibola" (Ypsilanti, Michigan) in that an economic elite maintained political control and, in fact, after 1884, actually solidified their domination over city politics.



Despite the soundness of methodology and thoroughness of research, Watts's study underscores some of the problems associated with primarily quantitative analyses of politics and society. First, more needs to be said about the electorate and the various issues that may have had an impact on winning and losing. Second, while Watts placed the candidates in Atlanta's political context, he did not discuss the Georgia or southern political context. What impact, if any, did Tom Watson and the Populist movement have on Atlanta politics and on the public's perception of the candidates? Third, considering the continuity in the social bases of Atlanta politics, a brief comparison with Floyd Hunter's study in 1953 of the city's decision makers might have been useful. Finally, the analysis of political elites does not respond to the important questions of power and community influence. The best indicator of "who governs" is how they govern. Study of the decision-making process and of the policies emanating from that process, as historians David C. Hammack and Carl V. Harris have demonstrated recently, is a more appropriate technique than political prosopography for determining the relative influence of various social groups in urban society.

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THOMAS G. ALEXANDER. *A Clash of Interests: Interior Department and Mountain West, 1863-96*. Provo, Utah: Brigham Young University Press. 1977. Pp. xii, 256. \$11.95.

This is a detailed study of the relationship between the interior department and three western territories: Idaho, Utah, and Arizona. The author has selected these three as a representative cross-section of what he correctly sees as a "colonial West." In analyzing the relations between these territories and the interior department, the author presents an exhaustive, if not exhausting, picture of day-by-day territorial happenings. Indeed, the overall story of the clash of interests between these territories and the federal government is, for the most part, lost in the trivia of figures on salaries, paper costs, transportation allowances, building rent, and so forth. This is unfortunate because, although the author struggles to bring to the forefront such important questions as clashes over land use and Indian policy, he appears not to have distanced himself sufficiently from his notes to give these issues a clearly defined dramatic outline. In a sense, the author is a victim of his own extreme conscientiousness and research, and this study, when patiently read, does yield significant and important insights. Among the important insights emerging from the book is the

fact that the interior department was governed by an ideology that tended to "view the far West as an underdeveloped and perhaps recalcitrant but nevertheless nascent Midwest." The author clearly demonstrates that this ideology conflicted with western realities, both geographical and sociological. More important, he argues convincingly that "traditional American ideology, which opposed monopolies and demanded equality of opportunity and which insisted that the homesteading agrarian was the highest type of American life, flew in the face of western conditions." This theme runs through the book and is documented by a myriad of detailed instances. This reviewer wishes only that the author had gained a better command of historical foreground-background perspective and that, having gained this command, had included all of the territories in his study.

WILLIAM H. GOETZMANN  
University of Texas,  
Austin

R. HAL WILLIAMS. *Years of Decision: American Politics in the 1890s*. (Critical Episodes in American Politics.) New York: John Wiley and Sons. 1978. Pp. xi, 219. Cloth \$10.95, paper \$6.95.

*Years of Decision* is one volume of a series of interpretive essays by various historians, under the general title *Critical Episodes in American Politics*, designed "to bring the new scholarship to bear" on major episodes in American political history.

R. Hal Williams traces national political events from the presidential election of 1888 to McKinley's assassination in 1901, synthesizing the current literature as well as using many contemporary sources. He offers the most thoroughgoing statement to date of what might be called the Republican-nationalist interpretation. The Republicans, in Williams's words, the "party of activism and national authority . . . seemed better suited to the needs of an increasingly interdependent, urban-industrial society" than the Democrats, with "their tenets of localism and negative government." Democratic strength early in the decade was a reaction against Republican "pietistic reform," manifested particularly in the "billion dollar Congress" (1889-91), but the Republicans had "learned from defeat," adopting "an expansive pluralism in which all religions, sections, and classes could take shelter" (p. 96). Readers of the newer literature on this era's politics will recognize this synthesis of the work of H. Wayne Morgan, Paul Kleppner, Richard Jensen, Samuel McSeveney, and others.

The voting behavior studies Williams uses, with their positing of a pietistic-liturgical continuum, fit rather awkwardly with his Republican-nationalist reading of national politics. Somehow an economic



depression shifted the onus of pietism from Republicans to Democrats. Williams's explanation of this shift is lame at best. He has both the elections of 1890 and 1896 ranking "among the most important in American political history" (pp. 47, 97). "Somehow," he remarks on page 53, "since 1888's lifting victory, Republicans had lost touch with the people." They then re-established this touch for a generation as Bryan lost votes "in the same way the Republican pietists had earlier in the decade" (p. 125). Can this distinction between pietists and liturgicals really apply to a national politics that elected to the presidency such an unbroken string of exhausting moralists from both parties between 1884 and 1920? In fact, after Melvyn Hammarberg's remarkable book on Indiana voters, one can reasonably doubt the significance of this dichotomy on any level of politics in the period.

Williams exaggerates the differences between the major parties, much as Matthew Josephson's generation exaggerated their similarity: in contrast to Republican activism, the Democrats are unrelievedly negative in outlook, their ideology "as suited to stalemate as to victory" (p. 5). Yet divisions within the parties were at least as sharp as divisions between them, and this was particularly true of the amazingly heterogeneous Democratic Party. Even Grover Cleveland was widely identified with reform and enlisted vigorous support in 1892 from such active sorts as John Peter Altgeld, Eugene V. Debs, Henry George, and William Jennings Bryan. There were many more ambiguities in the positions of both parties throughout the decade than Williams presents (and far more to be said about the People's Party than he has space to permit). Still, this is an interesting statement of how many historians view an important period in American political history and a useful book with which to start students arguing.

ROBERT D. MARCUS  
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Stony Brook*

GORDON B. MCKINNEY. *Southern Mountain Republicans, 1865-1900: Politics and the Appalachian Community*. Chapel Hill: University of North Carolina Press. 1978. Pp. xiv, 277. \$18.00.

Gordon B. McKinney has assembled a detailed narrative of Republican factional strategies and struggles in the Southern mountains in the late nineteenth century. The fruits of his research will be useful to historians of the South and of the Gilded Age. Of more general interest is his attempt to fit his narrative into a framework provided mainly by Robert H. Wiebe. McKinney's principal purpose is to explain a trend of growing Republican strength over the period in terms of a defense of local com-

munity interests against "forces created by the rapid industrialization of the Gilded Age" (p. 76). This phrase, which refers to the 1870s, is an exaggeration when applied to the South, as is his statement that the Southern mountains "faced the full impact of the American industrial revolution in the 1890s" (p. 128). How would McKinney describe Pittsburgh in the 1890s, or the TVA later? At any rate, he attributes growing GOP strength in the mountains from the mid-1870s through the 1890s to the rise of bosses leading well-organized "party-armies" and promoting local interests: veterans' pensions, seeds from the agriculture department, subsidies for local governments and businesses, and general justification for the wartime sacrifices of Unionists. It is not clear, incidentally, how Tennessee Republican leader L. C. Houk's accepting money from an L & N Railroad official makes Houk a "champion of local interests." McKinney notes that defense of community values and interests was characteristic of the whole country in this period, but does not ask whether Southern mountain Democrats were using the same sort of strategy, as they probably were. In that case Republican strength would have to be explained further.

The early 1890s were a turning point because of the threats posed by the Lodge elections bill and by the economic depression. The now old-fashioned "party-army" bosses were displaced by businessmen who took over the mountain Republican party and began "the systematic exploitation of the people and resources of Appalachia" (p. 128). This transition is the strongest part of McKinney's interpretation: the new GOP leaders used protectionism to build up the party during the depression by appealing especially to voters in mining and urban areas. This leaves the problem, however, of whether exploitation was therefore popular or merely cleverly concealed.

In his statistical appendix the author notes that Southern mountain Republicanism correlates best with an absence of black people. Further reflection on this point might have yielded rewarding insights. He indicates that racial policy was a crucial difference between the parties, but shows that Republican voters were not exactly liberal on race. Perhaps in a future work he will be able to assess the relative importance of racial, economic, and other factors to determine why some Southern mountain men became Republicans while others were Democrats.

ROGER L. HART  
*Department of State*

L. GLEN SERETAN. *Daniel DeLeon: The Odyssey of an American Marxist*. Cambridge: Harvard University Press. 1979. Pp. viii, 302. \$14.00.

In a 1976 article, L. Glen Seretan noted the scarcity of reliable information about Daniel DeLeon's life before 1890, when he joined the Socialist Labor Party, and suggested that the gap might be filled by "a unifying interpretive concept as a guide to intelligent surmise." The metaphor he chose was "the Wandering Jew." In this, the first full-length, scholarly study of DeLeon in print, Seretan successfully presents myriad details of his subject's entire life as well as his writings and activities under that rubric. The man who emerges from these pages is complex but not quite three-dimensional. Seretan has focused so wholly on DeLeon that his supporters, adversaries, and organizations are all mere background. As a result, the portrayal of DeLeon is nearly bereft of context. There are, moreover, puzzling omissions. The crucial struggles in the late 1890s with the New York Jewish section of the SLP and with the "Kangaroo" faction are mentioned, not analyzed. And, in view of the importance of the Socialist Trade and Labor Alliance to DeLeon's changing attitude toward organized labor, it is surprising that that organization's periodical, the *Socialist Alliance*, was not used. Several important aspects of DeLeon's thought are also ignored.

Within the narrow scope that Seretan has chosen, however, the book succeeds. It is not a conventional biography. Only the first two chapters are organized chronologically; each of the remaining seven contains references to DeLeon's entire career between 1890 and his death in 1914.

Meticulous detective work in several countries enables Seretan to refute DeLeon's lies about his early life. He was born of Sephardic Jews in Curaçao, not of Spanish aristocrats in Venezuela; he never earned a doctorate in Europe; he was never a professor at Columbia. The many acts of dishonesty by a man who sacrificed much for his cause can be explained, says Seretan, as those of a man who yearned to belong to a community and yet cut his ties with the Jewish community, who found a "chosen people" in the proletariat while repudiating the source of that concept, who "wandered" from movement to movement and from theory to theory seeking a home and yet fatally damaged or left every organizational home he entered.

The bulk of the book describes DeLeon's attempts to work out the proper relation between the political and economic movements. Under his leadership the SLP first tried to forge an alliance with the Knights of Labor. That failing, he found theoretical justification for forming the STLA. By 1905 that organization was moribund, and so he helped to found the Industrial Workers of the World. Expelled from the IWW in 1908, he tried to move closer to the Socialist Party. The book ends with a tribute to DeLeon's theoretical acumen. DeLeon's

work, says Seretan, "provides a sound point of departure for thought directed toward humanizing industrial society" and has enriched American social thought (p. 219)—an astonishing statement inasmuch as a very large proportion of the quotations from DeLeon, which generously sprinkle the book, are predictions, not one of which came true.

AILEEN S. KRADITOR  
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RHODRI JEFFREYS-JONES. *Violence and Reform in American History*. New York: New Viewpoints. 1978. Pp. x, 242. Cloth \$12.50, paper \$6.95.

In his interpretive study, *Violence and Reform in American History*, Rhodri Jeffreys-Jones argues that liberal reformers have exaggerated violence to gain their political objectives. Focusing on industrial conflicts between 1903 and 1912, the author uses statistics on strike-related deaths to indicate that many groups overemphasized the actual level of class violence in the progressive era.

Two of the book's most informative chapters show how private detectives and labor spies exploited labor violence for profit. While most union leaders and socialists opposed violent confrontations, others emphasized these incidents to gain public sympathy. Employers also interpreted strike violence to their own advantage. Newspaper reporters sensationalized industrial conflict to increase sales. Finally, many progressives exaggerated the bloodiness of this conflict to advance the cause of reform. Theodore Roosevelt is portrayed as the exceptional reformer who sought the truth in order to respond to strikes fairly and effectively. The author cites Roosevelt's effective intervention in the 1902 anthracite strike as an example, but admits that the chief executive's blatantly anti-labor policy in western mining strikes contrasted strangely with his belief in "fair play."

Jeffreys-Jones believes that the reformers' inflated rhetoric about the threat of violence proved to be counter-productive, because conservatives turned this rhetoric against the cause of reform in the law-and-order "backlash" after World War I. The same reaction occurred, according to the author, when liberals exaggerated the violence of the ghetto uprisings during the 1960s.

The author's argument about the rhetorical exploitation of violence is biased in favor of moderate groups who lobbied for reform by effectively using peaceful tactics. Terrorists like the McNamara bombers damaged the cause of labor reform, but most workers who engaged in retaliatory violence (the militant western miners for example) were fighting for survival, not reform. The progressive re-

formers who investigated bloody strikes for the Commission on Industrial Relations in 1915 may have exaggerated labor violence to gain reforms, but their political failure should not be attributed mainly to the use of inflated rhetoric. The conservatives who checked labor reform and created the backlash in the 1920s were not merely engaged in a war of words. Reactionaries did turn inflated liberal rhetoric to their own advantage, but this was effective only after the balance of class forces had turned in their favor.

The author's research supplements previous studies of industrial violence with statistics on strike-related deaths in the progressive era. Though 308 people died in strikes between 1890 and 1909, Jeffreys-Jones reaches the astonishing conclusion that violent class conflict in this period was "imaginary." How many deaths would have established the reality of violent class conflict?

While the author's own research contradicts the assumption that class conflict was imaginary, he does produce evidence to support the point that strike-related violence was *relatively* insignificant in the U.S. Strike fatalities are only compared, however, with those of two other countries, France and Wales, and then the comparison is limited to just two years. A more extensive comparison of European and American labor violence is needed to support the thesis that class violence in this country has been grossly exaggerated.

JAMES R. GREEN  
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Boston

JOHN ALLEN GABLE. *The Bull Moose Years: Theodore Roosevelt and the Progressive Party*. (Series in American Studies.) Port Washington, N.Y.: Kennikat Press. 1978. Pp. xi, 302. \$15.00.

This judicious study both supplements and complements George E. Mowry's pioneering work of 1946. Grounded on the Roosevelt Papers in the Library of Congress and the Progressive Party records at Harvard, it also makes discriminating use of published works. In particular, it successfully integrates state and national developments. Opening and closing sections summarize the road to Armageddon and the collapse of the Progressive Party in 1916, but the core is a close-grained descriptive analysis of the years 1912-14. Organizational and structural problems, including those created by the need to resolve conflicting state and national views on prohibition, woman suffrage, and other issues, receive strong emphasis.

John Allen Gable also offers a suggestive analysis of voting patterns based on general census data. On balance, he confirms some older interpretations, en-

riches others, and advances several new ones of his own. He shows that the dispute over the controversial trust plank of 1912 resulted from a misunderstanding rather than a conspiracy and that the party's organizational strength "was directly proportional not to the strength of the progressive movement in each state . . . but to the number of experienced and able political leaders" (p. 51). The party's essentially middle-class base of support would probably have failed to expand substantially even if complete polarization of the two older parties had occurred. Progressive candidates fared best in urban areas with high growth rates and in expanding rural counties where property values were high. They drew greater support from prosperous than from impoverished farmers, more from Germans and Scandinavians than from other ethnic strains. They also had more appeal to old-stock farm youths, whose migration to the cities had loosened their party bonds, than to ethnic blue-collar workers in the East.

Gable further contends that the Progressives were more forward- than backward-looking. An especially illuminating chapter gives an insightful analysis of the Progressive Service, an educational, programmatic, and legislative reference division that dominated party activities in 1913. He concludes, however, that the electorate's obsession with bread-and-butter issues induced by the depression of 1913-14 confirmed many Progressives' pre-Keynesian notion that a just and humane polity depended ultimately upon a "social surplus" of wealth.

Gable's treatment of Roosevelt, who figures centrally in some chapters and marginally in others, is at once appreciative, critical, and understanding. He asserts that T. R.'s acquiescence in Lily Whitism "compromised himself and the New Nationalism for a mess of pottage" (p. 74). He exposes the contradictions and paradoxes of many of Roosevelt's positions. And he makes a persuasive case for T. R.'s ideological dedication: "Committed to the vision of an organic commonwealth, Roosevelt declined to make interest group appeals, to appeal to labor as labor or to the poor as the poor" (p. 130).

WILLIAM H. HARBAUGH  
University of Virginia

JACK C. LANE. *Armed Progressive: General Leonard Wood*. San Rafael, Calif.: Presidio Press. 1978. Pp. xvi, 329. \$16.95.

Not since Hermann Hagedorn's sympathetic *Leonard Wood: A Biography* (1931) has there been a biography of the controversial general. Jack C. Lane's critical study focuses almost entirely on Wood's public life. He depicts Wood as a major figure in

the key developments of the early twentieth century. Beginning his army career as a contract surgeon in the 1880s, Wood became commander of the Rough Riders in 1898, military governor of Cuba one year later, commander of the Philippine Division in 1906, and by 1910 army chief of staff. Lane attributes Wood's rise to ambition and drive, the influence of political supporters such as Theodore Roosevelt and Henry Cabot Lodge, and administrative ability, a talent much in demand in a period of empire. Disappointment followed, however, when the Wilson administration not only refused to appoint Wood commander of the AEF, but also denied him any military service in the European theater. Then, as an exponent of 100 percent Americanism, Wood failed to win the Republican presidential nomination in 1920. Afterward, President Harding appointed him governor general of the Philippines, where he languished until his death in 1927. Lane imputes Wood's reversals to a self-righteous, rebellious, and excessively ambitious nature that Wood especially revealed in challenging Wilson on the preparedness issue.

Like Roosevelt and Lodge, Wood was a Social Darwinist, an expansionist, and a progressive. As a Social Darwinist, Wood believed in the inevitability of conflict and in the supremacy of the Anglo-Saxon race (Cubans and Filipinos were "stupid, downtrodden people"). As an expansionist, he believed that the United States should retain Cuba and the Philippines. As a progressive, Wood provided for "efficient improvement of schools, courts, local governments, hospitals" in Cuba. He also sought military reforms that embodied the principles of scientific management. As chief of staff, Wood strengthened that office and improved the efficiency of the army.

Lane acknowledges Wood's accomplishments, including the eradication of yellow fever and the improvement of public works in Cuba, the winning over of the nation to the conscript idea, and the training of troops for the war. But Lane emphasizes more Wood's limitations, for example, his inability to understand other cultures, causing a number of his colonial reforms to fail. More important, Wood was an intolerant autocrat who often challenged authorities who stood in his way. Revealingly, perhaps, his greatest admirer was Douglas MacArthur.

My major criticism is that Lane fails to make a case for Wood as a progressive. Surely Wood's limited "progressivism" does not justify the book's title. Lane himself hedges on Wood's progressivism in Cuba. And he could well have labeled arch-conservative Secretary of War Root a progressive if Lane had employed the same rationale as he did for Wood regarding military reform. Wood clearly was a conservative, if not a reactionary, as a presidential candidate in 1920.

Too, Lane could have probed the young Wood's relationship with his father, who seems a key to understanding Wood. There is also no mention of Wood's mother nor his wife and son after 1891. Nevertheless, even though it also ignores such potentially useful manuscript collections as the Taft, Pershing, and Lodge Papers, this competent book deserves the attention of scholars.

JAMES N. GIGLIO

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THOMAS SCHICK. *The New York State Constitutional Convention of 1915 and the Modern State Governor*. New York: National Municipal League. 1978. Pp. xv, 148.

In this slender book Thomas Schick describes how the New York Constitutional Convention of 1915 came to include in its proposed constitution amendments providing for the short ballot, executive budget, and consolidation of administrative agencies—measures designed to strengthen and concentrate the administrative power of the governor. That a convention comprised mostly of conservative men of wealth would support proposals long advocated by political theorists and reformers Schick attributes, on the one hand, to the adroit management of the convention by Republican leaders Henry L. Stimson and Elihu Root and, on the other, to the convincing arguments for reorganizing the executive branch presented by Stimson and the New York Bureau of Municipal Research (BMR), especially its long-time members Charles A. Beard and Frederick A. Cleveland. Although the constitution was rejected by the Empire State's voters, the convention's work, Schick argues in an afterword, served as a catalyst for the eventual adoption by numerous states, including New York, of some form of executive reorganization.

The volume's major strength is also its chief weakness. Schick's narrow focus on the convention and its work allows him to organize his narrative chronologically and clearly, keep the chief actors in the forefront of the story, and sketch sharply the major arguments for and against the reform proposals. Yet he fails to place the constitutional convention and what he calls the reorganization movement into a sufficiently broader progressive framework. Thus, he fails to suggest the implications of fragmented state executive power for our notions about the extent of bureaucratic centralization actually accomplished by prewar progressives. Nor does he adequately relate his discussion of reorganization theory to a maturing progressivism that by 1915 had already accomplished much in the way of administrative and political reform at the city, state, and national levels and had given rise to a sophisticated body of ideas about modernizing government and politics.



Schick considers the three reorganization measures radical, but certainly these proposals were consistent with the philosophy and reforms long advocated by the business-oriented economy and efficiency wing of urban progressivism. The BMR, which embodied much of this philosophy, can hardly be considered a radical organization. Significantly, as Schick points out, a group of labor-related proposals, including one providing for old age pensions and one providing for a state fund for workmen's compensation, were rejected by a largely unsympathetic convention, prompting organized labor to campaign against the new constitution. Did not labor's proposals fit the reorganizers' definition of a modernized state government? If not, why not? Why, during the supposedly conservative twenties was Governor Al Smith able to secure approval of these same reorganization measures? These and similar questions could only have been addressed had Schick broadened his perspective by utilizing the rich and abundant literature on the Progressive movement and 1920s.

Only a few errors escaped the copy editor's eye. The more significant one is on page 16; "nineteenth century" is twice used (lines 5 and 13) when the context obviously requires that it be "twentieth century."

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Long Beach

JOHN P. DIGGINS. *The Bard of Savagery: Thorstein Veblen and Modern Social Theory*. New York: Seabury Press. 1978. Pp. xiii, 257. \$14.95.

Thorstein Veblen deserves to be considered a major theorist of modern capitalist society. John P. Diggins successfully locates the area of Veblen's thought that ranks him with figures like Marx and Weber—Veblen's understanding of how ancient, historically conditioned "habits of thought" (modern social scientists would say "status norms" and "codes of behavior") govern modern American society. Diggins shows that Veblen's explanation of how emulation creates and perpetuates a kind of "false consciousness" is analogous in some respects to Marxist concepts of alienation, reification, and hegemony; and he suggestively links the view of American social structure that emerges from Veblen's analysis to themes introduced by earlier theorists of America like John Adams and Alexis de Tocqueville.

Although the book conveys important dimensions of Veblen's strength, the untutored reader will find the analysis misleading at times. Diggins rightly points out the ambivalence in Veblen's psychology, as in other aspects of his thought, between nativism and environmentalism, romanticism and

mechanism. But he does not make clear how Veblen attempted to bridge those opposites. Following Herbert Spencer's cue that what was native in the individual had been acquired by experience in the race, Veblen argued that the deepest layers of human nature, and the soundest, were laid down over the long course of savage history by environmental conditioning and were safely embedded in biological instincts and propensities. Far from being unaware of the social implications of Dewey's psychology, as Diggins charges (p. 56), Veblen implicitly dissociated himself from them in his 1906 essay on "The Place of Science in Modern Civilization" and turned instead to William James and Jacques Loeb for support of his biological nativism.

Diggins rightly stresses the importance of Marx to Veblen. While Veblen rejected Marx's psychology, he took over Marx's view of history as the systematic development of class domination. But Diggins argues that Veblen was not concerned, as Marx was, with class as related to the means of production, but rather was concerned with status as shaped by consumption and styles of life (p. 88), thus separating what Veblen saw as conjoined. The failure to see the persistence of direct class domination within Veblen's analysis leads Diggins to confuse Veblen's understanding of power and the bearing of his work to modern social theory.

These missteps may relate to the author's method. Diggins "confronts" Veblen with the major social theorists of modern society—"Marx, Weber, Tocqueville, Durkheim, Simmel, Sombart, Mead" (p. viii)—and a score of others from Hobbes to Menckel. With so wide a cast of characters, lines of analysis are sometimes hurriedly taken up and surface features sometimes predominate. Moreover, Diggins believes that this theoretical task is exempt from the effort to place Veblen in historical context. That effort, he states at the outset, has already been made and shows only Veblen's relation to the popular reformism of the day. But social theory too has a historical context. One would never guess from this book that one of the most important theoretical influences on Veblen came from Herbert Spencer. Nor could one guess what questions Veblen asked of himself and what dialogue they were intended to advance. The theorists examined and the questions asked of the material are generated not by history but by the author's theoretical concerns. They enlarge our vision of the historical Veblen, but also blur it.

DOROTHY ROSS  
University of Virginia

CARLTON O. WITTLINGER. *Quest for Piety and Obedience: The Story of the Brethren in Christ*. Nappanee, Ind.: Evangel Press. 1978. Pp. x, 580. \$12.95.



This is a fine book, painstakingly written by a professional historian who is also a leader in the Brethren in Christ church. It is superbly researched and clearly written. The author is an insider who understands the intricacies of German sectarian theology, but he also relates his subject to broader historical and sociological questions. He writes objectively, telling the story, as he says, "without praise or blame."

The Brethren in Christ church was a small sect born among the German settlers in Pennsylvania in the late eighteenth century. Closely akin to the Mennonites, the church remained separate from the larger German sects because of its distinctive teachings on a few issues, most notably baptism. At the end of a century's existence the church had expanded geographically to several states and Canada but still claimed only about two thousand adherents. In spite of growing missionary efforts in the twentieth century, in 1975 the church numbered only about twelve thousand five hundred members.

Carlton O. Wittlinger divides the story of the Brethren in Christ into four parts. The period up to 1880 saw the full development of the church's doctrine of nonconformity and a slow numerical expansion. During the years from 1880 to 1910 the church became more aware of outside religious developments and was increasingly influenced by evangelical Protestantism, particularly the holiness movement. These influences mellowed the group but left unchanged its distinctive nonconformist practices. Wittlinger calls the years from 1910 to 1950 a "period of adjustment." By 1950 the church had become much less rural and less sectarian. At the same time, the Brethren in Christ remained preoccupied with old issues; the church's most heated debates centered on dress codes. Since 1950 a period of rapid change has swept the church into the mainstream of American evangelicalism.

The only serious question the book raises is whether such a small church deserves such extensive treatment. Probably few people outside the Brethren in Christ will be interested in the details of the story. And yet it is the complexity of their debates that brings life to this American sectarian culture. This is a model study of a community born in a fanatical search for Biblical consistency slowly coming to terms with the society around it.

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Birmingham

JOHN PERRY. *James A. Herne: The American Ibsen*. Chicago: Nelson-Hall. 1978. Pp. x, 343. Cloth \$17.95, paper \$9.95.

John Perry's biography of James A. Herne offers the fullest treatment yet of this important late nine-

teenth-century American actor and dramatist. Perry's extensive factual account of Herne's life and works will require attention from students of theatrical and dramatic history. It may also interest those who see the theater in the wider contexts of social and cultural history, but in this aspect the book will almost surely disappoint.

Even determined readers may be put off by Perry's distracting style. His fondness for alliteration, answered rhetorical questions ("Why did performers like Lola Montez continue to perform under such conditions? For money" [p. 32]), and inept metaphors ("Garland paid 35 cents to perch in the first balcony of the Boston Museum and inhale Booth's liquid diction" [p. 106]) sometimes get annoyingly in the way of one's wish to take him seriously. Attempts to set the social scene by means of a vivid characterizing impression often fall flat, as in his description of the lively San Francisco of the 1870s: "By 1879 around 6,000 young bloods roamed across San Francisco, personifying Victorian excess" (p. 35). Such lapses betray a more important shortcoming, Perry's unsatisfactory command of certain contexts in which Herne's life might be placed. He asserts, for example, that Herne's early play *The Minute Men* was influenced by "the many cross-currents of nineteenth century American romanticism" (p. 66) but leaves us with only a mention of Fenimore Cooper and a few other sources and no real explanation of "crosscurrents."

The author stands on surer ground in describing the theatrical milieu in which Herne moved, first as actor and later as playwright. In temperament and physical attributes Herne was well equipped to survive and even flourish as a performer and writer in the demanding world of despotic managers and entrepreneurs, equally despotic popular audiences, and season-long crosscountry tours. Viewed in these trying circumstances, Herne's emergence as a dramatist with an authentic vision of contemporary domestic life becomes all the more remarkable. That he should have gone on to write *Margaret Fleming*, a shocking exposure of the "double standard" underlying modern marriage, indicates how genuine Herne's talent was for precise analysis of human nature and reveals as well his utter moral integrity. Perry must be credited with providing these helpful details of Herne's personal and professional qualities and their resulting impress on the history of American theater and drama.

Evidently, Perry is much better at dealing with persons and facts than with less concrete matters. His chapter on the influence of the young radical Hamlin Garland on the impressionable, older Herne draws a balanced, judicious portrait of Garland, pointing up Garland's introduction of Herne to advanced social thinkers and avant garde dramatists of contemporary Europe. The chapter on

Henry George and his friendship with Herne, who under George's influence became a life-long champion of social justice, is similarly successful.

Herne's life and writings, it is clear, constitute an unusual claim on our attention by a man of the theater. Despite his faults as a writer and historian, Perry establishes this point reasonably well. Unfortunately, the claim he makes for Herne in the subtitle of the book seems unjustified. The epithet may have served in Herne's own day to characterize his extraordinary stage realism, but at this late date it only trivializes its subject. Although Herne's rejection of outmoded theatrical conventions in favor of a consistent, unglamorous imitation of actual human behavior and environment appears to link him to the infamous Norwegian, he did not share Ibsen's profound philosophical anarchism or, for that matter, his towering dramatic genius. To call Herne "the American Ibsen" is to overvalue the considerable contribution Herne made in his own way to American popular entertainment and to the rise of realism as a potent force in American life.

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Amherst

TIMOTHY K. NENNINGER. *The Leavenworth Schools and the Old Army: Education, Professionalism, and the Officer Corps of the United States Army, 1881-1918.* (Contributions in Military History, number 15.) Westport, Conn.: Greenwood Press. 1978. Pp. 183. \$15.95.

With the exception of West Point, historians of the United States Army have largely ignored the rise of the service schools and their role in the professionalization of the officer corps. As Timothy K. Nenninger notes, however, formal education is more important to the military profession than to other groups "because of the limited opportunities officers have to practice their profession—to command and manage troops in combat" (p. 6). His short monograph on the origins of the army schools at Fort Leavenworth is a significant step toward filling this gap. He traces the evolution of the Leavenworth schools in the general context of military reform at the turn of the twentieth century and assesses their influence in preparing the army for the complexities of modern warfare.

Originally established in 1881 as a "school of application" for infantry and cavalry lieutenants, the Leavenworth institution had little immediate impact on the army. The curriculum emphasized routine recitations on tactics and military administration; efforts to remedy the basic educational deficiencies of many young officers gave the school a reputation as the "kindergarten" of the army. During the 1890s, however, innovative instructors, Arthur L. Wagner and Eben Swift, introduced Ger-

man techniques of military education, especially the "applicatory method" of teaching tactics through the solution of concrete battlefield problems. The reform of the army after the Spanish American War, which included the adoption of the modern general staff system, encouraged the growth of military education. Two principal schools emerged at Leavenworth: the School of the Line, which emphasized tactics and other subjects related to the command of troops in the field, and the Army Staff College, which trained the superior graduates of the School of the Line in strategy, logistics, and staff procedures. While the Leavenworth schools slighted the impact of changing technology on warfare, they instilled in their students a respect for professional study and careful, systematic planning. The competent performance of Leavenworth graduates in key staff positions during World War I contributed greatly to the schools' reputation and to the acceptance by the officer corps of advanced professional education as a criterion for military leadership.

Thoroughly researched and judicious in its analysis, this work represents recent trends in military historiography in its use of comparative and social scientific perspectives, particularly the concept of professionalization. Nenninger does not always develop these ideas fully, however, or integrate them into the body of his narrative. For example, he draws parallels in his first chapter between the consolidation of the military profession and such general trends of the Progressive Era as bureaucratization and the rise of scientific management, but he does not explore this relationship. Although he includes tables indicating the representation of Leavenworth graduates on the General Staff and their wartime assignments, he might have examined more thoroughly the personnel of the schools—the characteristics of officers assigned to Leavenworth, the selection process, and the subsequent career patterns of graduates. This book is, however, informative and clearly written and makes an original contribution to the institutional history of the United States Army.

WILLIAM B. SKELTON  
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Stevens Point

THOMAS J. NOER. *Briton, Boer, and Yankee: The United States and South Africa, 1870-1914.* Kent, Ohio: Kent State University Press. 1978. Pp. xiii, 192. \$12.00.

Studies of relations between the United States and Africa, particularly black Africa, are rare; books dealing with relations with South Africa are virtually nonexistent. The fact is, of course, that compared with other large areas of the world, official relations between the United States and Africa have

been slight. Nevertheless, because of the growing importance of Africa in world affairs and because, too, of the large black population in the United States, the need exists for studies of African-American relations.

The main virtue, and it is not a negligible one, of Thomas J. Noer's book is that it is the first one on the subject. In 142 pages of text, including a conclusion, Noer outlines events from 1870 to 1914. Clearly so short a study of forty-four years could not pretend to be anything but superficial. Moreover, the book is based only on American archival material, and a rather limited selection of it at that. There is voluminous additional information in the British Public Record Office and presumably in South African archives as well, material essential for a thorough study.

Noer's thesis is that, until several years after the Boer War, the United States backed British imperialistic control of South Africa. It did so for two principal reasons. First, due to the growing "surplus," Americans needed new overseas markets, and they thought that South Africa, if only Great Britain took over from the narrow, backward-looking Boers and modernized the area, would become a significant market. Second, American missionaries in South Africa believed that enlightened British rule would favor the spread of Christianity among the blacks, whereas the Boers, persuaded that the blacks were inherently inferior, opposed any moves toward uplift. Thus the United States supported Britain during the Boer War and welcomed the later creation of British South Africa. But American policy failed. After Britain defeated the Boers, American exports to South Africa fell sharply, and black Africans remained as oppressed as before. Indeed, after the war American criticism of segregation and of the beginnings of apartheid increasingly supplanted the older attention to commerce and missionaries.

Although there is something to be said for this thesis, too much weight is given to it, and it makes American policy appear overly coherent. Could Washington really have attached much importance to the small potential market in South Africa? It presented a marked contrast to the Chinese potential market, teeming with 400 million supposedly eager customers. And did Washington really pay much attention to the views of American missionaries in South Africa? According to Noer, American policy during the Boer War "was the logical continuation of two decades of assumptions about the future of South Africa" (p. 70). It is doubtful that such continuity and rationality existed. One suspects, for example, that if John Hay had not been Secretary of State during the war, official attitudes would have been considerably less pro-British, mar-

ket and missionaries notwithstanding. That the two previous decades had not convinced the general public is suggested by the fact that the Democratic platform of 1900 extended sympathy to the Boers and the Republican platform came close to doing so. Related to the matter of coherence is the question of who was coherent. Noer is fond of such statements as "the United States determined that British imperialism rather than continued Boer independence was most compatible with American concepts of 'progress'" (p. 135). Who determined—the administration, the general public, someone else?

There are some interesting sidelights: American engineers controlled more than half of South African mines; Americans had an important part in the Jameson raid; after the Boer War hundreds of British soldiers applied to join the American army in the Philippines; Washington collected and forwarded to Tokyo detailed descriptions of labor conditions on the Rand in order to illustrate the comparative benevolence of American treatment of the Japanese. Altogether Noer has written a useful preliminary study that will help others to make more penetrating analyses.

CHARLES S. CAMPBELL  
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RACHEL WEST. *The Department of State on the Eve of the First World War*. Athens: University of Georgia Press. 1978. Pp. 183. \$11.50.

In his well-known book, *American Diplomacy, 1900-1950*, George Kennan described the state department at the turn of the century and even in the 1920s as a rather drowsy environment. It was "a quaint old place, with its law-office atmosphere, its cool dark corridors, its swinging doors, its brass cuspidors, its black leather rocking chairs, and the grandfather's clock in the Secretary of State's office. There was a real old-fashioned dignity and simplicity about it" (p. 80). It is an image that must be compellingly attractive, for it has surfaced again, this time as the focus of an entire book about the department on the eve of World War I.

This slim volume (162 pages of text and notes) represents an effort to provide a "photograph" of the Department of State in 1913-14. After briefly summarizing aspects of the department's development over a century and a quarter, Rachel West devotes succeeding chapters to Secretary William Jennings Bryan, Woodrow Wilson and Colonel House, the department proper, ambassadors in London and Berlin, "Ambassadors Elsewhere" (in Europe), and "Ministers and Secretaries" (again in Europe). A concluding chapter summarizes major events during the summer of 1914; in it the author

wonders whether the United States could not have taken the initiative for peace.

Throughout, West is highly critical of America's foreign affairs machinery, which she considers "antique in size and arrangement" (p. 4). "The sheer amateurism of the department, the blindness of the highest officials—these stand out" (p. 3). She compares the size of the department then and now and finds the smaller organization wanting. A more appropriate comparison would have been with contemporary foreign offices; although the department in 1914 had but 213 employees, the British Foreign Office had still fewer—176. And on her next-to-last page West concedes that the European foreign offices were probably no better prepared for the outbreak of war than was the state department.

One would not know from this book that the period between the 1890s and World War I actually was crucial to the development of the modern United States foreign service. During that time the diplomatic, consular, and departmental branches were reorganized, for reasons involving chiefly marketplace expansion and bureaucratic entrepreneurship. The consuls and diplomatic secretaries were placed on the merit system, where they have remained. The department, among other things, adopted the principle of geographic specialization, linked the legations and embassies to the department via a new information division, tied the consulates to Washington by an inspection system, and became for American businessmen an important source of information and encouragement about foreign markets. West has very little to say about these developments.

It is almost a truism to observe that American foreign policy officials before World War I were much more concerned with economic (chiefly commercial) expansion into Latin America and the Far East than with the European balance of power. They took the latter for granted. If West wished to examine the department's modernity or lack of it in 1913–14, she would have done well to include in her coverage parts of the world that, as she acknowledges (pp. 19–20), received more attention from the United States government. At the same time, she might have consulted the extensive 160 series of the department's decimal file, which houses the material relating to organization for foreign trade relations. She did neither of these things. Yet any account of the American foreign service in this period that virtually ignores the consular service, Latin America and the Far East, economic expansion, and the state department's relations with the commerce department is partial at best.

To give West her due, there still were problems in the foreign service in 1914, particularly in the diplomatic corps, and she provides useful information

about these, about some foreign service officials, and about the problems of assisting Americans caught on the Continent in late July and August 1914. Particularly good is a lengthy sketch of John Bassett Moore as the department's counselor and second-in-command under Bryan. Moore has been neglected by historians, and West performs a real service by bringing him out of the shadows.

Nevertheless, by adopting a "great departure," Eurocentric framework, West's account provides a skewed view of U.S. foreign relations on the eve of the First World War.

RICHARD HUME WERKING  
*University of Mississippi*

DRAGAN R. ŽIVOJINOVIĆ. *The United States and the Vatican Policies, 1914–1918*. Boulder: Colorado Associated University Press. 1978. Pp. 240. \$12.50.

The world has become smaller and international scholarship seeks to hurdle the boundaries of national bias. Dragan R. Živojinović of the University of Belgrade did research in Europe and the United States for this book published in Colorado. He amply documents the general naiveté and overall failure of Vatican diplomacy during World War I. Benedict XV and his chief minister, Pietro Cardinal Gasparri, sought to promote an early end to the war but failed. They sought to keep the United States out of the war but failed. They hoped for a seat at the peace table but were disappointed. Above all, argues the author, they strove to prevent the collapse of Austria-Hungary; again they failed.

Yet in demonstrating these facts the author reveals a logic that raises questions of national bias. Živojinović simply assumes that Vatican desires to preserve Austria-Hungary deserve condemnation. He also argues that Vatican attempts to keep the United States out of the war prove that the pope was the tool of the Central Powers. Finally, he blames the Vatican for helping cause World War I by "sanctioning" the attack on Serbia. Such views give World War I a moral simplicity that belie the volumes of debate published over the last fifty years.

Throughout the book, written in an argumentative style, the author lapses into contradictions. At one point he writes that the Vatican was an Italian institution working for Italian interests (p. 3). Later he presents evidence that the pope sought to increase his world-wide influence and hoped to liberate the Vatican from Italian domination. At another place the author writes that the pope was reconciled to German domination of Europe (p. 34). A few sentences later, we read that the "pope was intent on preserving the status quo ante bellum in Europe."



Most damaging is the author's one-sided approach to the key problem in any study of Vatican-United States policies. What prevented the collaboration of the world's two foremost neutralists? At first the author argues that Wilson rebuffed papal appeals to remain neutral because the United States recognized that the Vatican's main purpose was to save Austria-Hungary. Živojinović seems curiously blind to other obstacles to cooperation for peace. The Presbyterian Wilson had difficulty dealing with the representative of Roman Catholicism for purely personal reasons. In addition, Wilson's own pontifical complex stood in the way of collaboration. Colonel House recognized that Wilson wanted center stage for himself. Wilson spoke from his own moral pulpit when he rejected the papal peace plan of August 1917, because he felt German leadership was "morally bankrupt." A key part of Wilson's revelation called for the promotion of self-determination by national groups, excluding the Irish and assorted Asians, whose fate was being determined by the Allies. The papal peace plan differed from Wilson's Fourteen Points only in this area of self-determination. The issue assured that Wilson would refuse collaboration with Vatican officials seeking to preserve the conglomerate called Austria-Hungary.

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JOHN W. HEVENER. *Which Side Are You On? The Harlan County Coal Miners, 1931-39*. Urbana: University of Illinois Press. 1978. Pp. xiv, 216. \$10.95.

From 1931 to 1939, when the coal operators of "Bloody Harlan" County finally agreed to make the United Mine Workers of America sole bargaining agent for all of their employees, thirteen residents died in the struggle over union recognition. Florence Reece's folksong, from which John W. Hevener takes his title, has become a classic. Hevener characterizes the decade of strife not just as a fight for recognition and better working conditions but as a "miners' revolt against the Harlan mine owners' arbitrary economic, political, and social power" (p. 14). The miners won the contest, he argues, only because the New Deal "created a new balance of economic and political power in Harlan as it did throughout the nation" (p. 180).

On this central thesis, Hevener is convincing. He shows how the National Recovery Administration's labor boards failed to affect the power structure before 1935; then the Wagner Act gave New Dealers the tools to haul some of the most disreputable anti-union coal operators into court. The La Follette Civil Liberties Committee exposed the key element in the operators' system of intimidation, the private

mine-guard system, and aroused reformers to pressure the Kentucky legislature to abolish it. Stripped of their "gun thugs" and squeezed by an alliance of northern operators, UMW President John L. Lewis, and New Dealer Frank Murphy, the Harlan operators finally capitulated.

Hevener takes his reader through much detail, some of it annoyingly repetitive, to other conclusions about an easily stereotyped series of historical events. He shows that the "side" combatants took was not a function of class, that the Harlan operators were themselves divided and not part of a large conspiracy, that the visit in 1931 by Theodore Dreiser's National Committee for the Defense of Political Prisoners prolonged a futile strike and thus contributed to miner unemployment and destitution, and that the La Follette Committee heard a good deal of perjured testimony.

Hevener accurately explains how the Harlan County mining industry was locked in a fierce competitive struggle with the unionized northern mines, a contest that strengthened both the Harlan operators' resistance to unionism and the miners' determination to fight for their rights during the New Deal. But Hevener does not emphasize the clout Lewis and the northern operators wielded. The alliance between Lewis and the operators in the Central Competitive Field exploited the New Deal's concern for economic recovery to lobby passage of the Guffey-Snyder Coal Act, a law that would have benefited unionized, northern operators at the expense of their Harlan County competitors. The Supreme Court invalidated this blatant piece of interest-group legislation. Thus, Harlan County operators faced a real, not an imaginary, conspiracy up north. The New Dealers, for their part, wanted to restore the health of the coal industry, not simply to protect the civil liberties of one group of miners, a point Hevener does not underscore. Oddly, Hevener did not consult the papers at the Franklin D. Roosevelt Library, which would have been useful on these matters. Still, *Which Side Are You On?* is a much more judicious study than the title might suggest. Indeed, Hevener's careful and well-written monograph convincingly illuminates the complexities of a fascinating chapter in American labor history.

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TAMARA K. HAREVEN and RANDOLPH LANGENBACH. *Amoskeag: Life and Work in an American Factory-City*. New York: Pantheon Books. 1978. Pp. xiii, 395. \$15.00.

In a compelling series of interviews, this volume recreates the work experience in the Amoskeag



Mills in Manchester, New Hampshire, from the high point of activity in the early twentieth century, when the company dominated the life of the city and the mills were the largest textile manufacturing center in the world, through the subsequent decline of the 1920s to the closing of the mills in 1936. The interviews concentrate on the relationship between work and the totality of life for the employee. Individually each interview is a fascinating glimpse into the distinctive life of a worker; collectively the interviews reveal common threads in the experience of these first- and second-generation Americans.

The reader becomes aware of the relative lure of factory employment in Manchester when compared to the even more grinding poverty, hopelessness, and boredom of rural life in Quebec; of how large families, the difficulty of foregoing income, linguistic and cultural barriers, and discrimination combined to block alternatives and lead the first and second generation into the mills; of the centrality of family relationships, and, one step removed, the importance of ethnicity as a basis for work experiences, including entry and continuity in the mills, as well as marriage and social contacts; of the company's domination over the lives of the workers and the resigned acceptance of this situation by most employees until the 1920s; of the subsequent growth of opposition to the company's policies during its period of decline as pressures mounted for more work at less pay, and how this led to efforts at unionization and to several bitter strikes; and of how important work was as a confirmation of personal worth and as a measure of one's ability to cope with life. For some, necessity could become virtue as well. As one worker put it: "When you work twelve hours per day, you have to find pleasure in work. There is nowhere else to find it" (p. 231).

This book fits within the current effort to reconstruct the lives of American workers as distinct from the history of the labor movement. Its success depends on how well the elderly interviewees could remember and express what took place so many years before. They do both very well. Readers familiar with industrial conditions in the factories of early twentieth century America will not be surprised by what the workers from Amoskeag recount. Yet the book has great value because one carries away a genuine understanding of how individuals responded to the seemingly implacable demands and problems of poverty, large families, inadequate education, and constant labor in the mills.

There are several weaknesses that result from decisions about concept and form. First, the authors should have expanded the interpretive and explanatory material (about 10 percent of the whole) even if it meant omission of some interviews. This is so even though Tamara K. Hareven intends to pub-

lish a complementary volume on how immigrants adjusted to industrial life. Second, there are only a few comments about the interviews beyond biographical headnotes. Yet, as the authors point out, it is important to evaluate interviews in terms of why certain material has been recalled and how it has been expressed. The book would have gained from the authors' insights on these points. Finally, the lack of an index detracts from the volume.

Despite these few limitations, the volume provides a rich store of information about first- and second-generation industrial workers and a real sense of what their lives were like. The success of this book, and the realization that so many of those interviewed were near the end of their lives, should spur other projects to recapture the experiences of workers before their valuable commentary is lost.

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JACQUES M. QUEN and ERIC T. CARLSON, editors. *American Psychoanalysis: Origins and Development*. New York: Brunner/Mazel. 1978. Pp. xii, 216. \$15.00.

This collection of twelve essays by psychiatrists and intellectual historians originated as the Adolf Meyer Seminars in the Section on the History of Psychiatry and the Behavioral Sciences of the Department of Psychiatry of the New York Hospital—Cornell Medical Center. Something of the starchy, formalistic milieu evoked by that title clings to the book, with close to half the space given to capsule histories of theoretical schisms and the formal institutions that so often reflected them. There is not a great deal in most of the essays by psychiatrists that will aid the historian who lacks interest in institutional minutiae. The exceptions include Roger L. Shapiro's careful study of ego psychology and the two final essays, by Earl G. Witenberg and by Arnold M. Cooper and Robert Michels, which provide rapid overviews of intraprofessional polemics over the field's scientific status and prospects that are too recent to have reached any but the most alert historian. Budding practitioners of psychohistory might acquire antitoxins to gullibility here.

For most readers of this journal the prime interest will reside in the four essays by professional historians. This judgment rests, I hope, on more than disciplinary chauvinism: one seems to find here a greater sense of the fluid, multivariate relations between professional imperatives, conceptual systems, and cultural environments. Hannah Decker, in a suggestive survey of "Freud and the Europeans," offers a comment by Freud himself that becomes a major theme in what follows: in the United States, Freud wrote, the two groups most enthusiastic

about analysis were "professors and superintendents of mental hospitals." Barbara Sicherman provides an excellent survey of the reception by "superintendents" and their pedagogical allies, stressing not only the role of Adolf Meyer in providing the theoretical stage for the drama of Freudian acceptance but also the broader importance of medical utopianism in creating a partly sympathetic attitude. The triumphs of scientific medicine and sanitary reform had led some spokesmen to espouse a millennial belief in the ultimate elimination of illness, somatic and psychic. This is an important perspective on the endless interpretive revisions of "progressive" reform. Indeed, it might be extended to suggest that the achievements of the new medicine made it an important role model for lay progressives in a variety of areas; the "centralizers" so trenchantly delineated by Samuel P. Hays and others may have found their model of expert therapy for social ills in medicine.

Dorothy Ross surveys Freud's "professors," in this case James, Hall, and the functional school of psychologists centered at Chicago, whose work provided a framework of conceptual reception similar to that which Meyer's work provided in the psychiatric field (Meyer had himself been influenced by the Chicago theories). In academic psychology, however, even more than in psychiatry, there was a rebound against analysis as the positivist drive for rigorous scientific method came to dominate the field. Ross's essay provides support for the suggestive comment by John C. Burnham that we will understand the reception of analysis better by viewing two distinct waves of influence: the first in the twenties, the second beginning largely afresh in the 1930s and peaking after 1940 with the "psychiatric deluge" in the mass media.

Burnham's richly suggestive chapter defies adequate summary. Twenty pages of text and valuable notes offer an outline of the rise of psychoanalysis to cultural power in the 1940s and 50s and suggest some reasons for its possibly brief conceptual dominance—for, as Cooper and Michels also note, there is considerable evidence that the broader cultural influence of analysis is declining. Burnham's stimulating suggestions might be enriched by noting that the major challenges to received opinion in the 30s and 40s, fascism and Nazism, defied explanation in utilitarian, interest-based terms and seemed to compel an exploration of the depths beneath conscious calculation. His hypothesis that the readiness of intellectuals to embrace analysis stemmed in great part from its ability to provide personal illumination, and perhaps relief, might be extended to account for the recent decline. In recent generations of intellectuals, changed childrearing practices and sexual mores may have reduced the numbers of

conflicted personalities who found Freud personally relevant. Action to change the world, rather than depth analysis, again became central. Burnham's stimulating essay will inspire many such reflections and, with the other articles noted, makes this mixed volume an important one for intellectual historians and those other historians of the twentieth century who are able to admit that not all is determined by infrastructure.

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JOHN TEBBEL. *A History of Book Publishing in the United States. Volume 3, The Golden Age between Two Wars, 1920-1940.* New York: R. R. Bowker. 1978. Pp. xiii, 774. \$32.50.

This 774-page tome, volume 3 of John Tebbel's monumental history of American book publishing, covers a crucial twenty-year era beginning in 1920, when the industry was still dominated by conservative, WASPish older houses, and spans two boom-and-bust decades marked by a revolution in literary standards, bitter censorship struggles, the development of innovative marketing strategies, and the ascendance of brash and ethnically diverse newcomers. Much of the cultural history of the interwar years and beyond can be extrapolated from these pages, particularly those dealing with such portentous events as Simon and Schuster's phenomenal debut with crossword puzzle books in 1924, the beginning of the Book of the Month Club in 1926, and the founding of Pocket Books—forerunner of the paperback revolution—in 1939.

Tebbel himself shies away from interpretation and selectivity of emphasis in favor of an encyclopedic factual approach; this vast compendium of names, titles, and dates (the index alone runs to seventy-one pages) definitely belongs in the reference section. Within the two major parts (one for each decade) are twenty-six chapters with numerous subsections, some examining specific facets of publishing history (censorship, advertising, and design and manufacture, for example) and others recounting the histories of individual houses, not only of trade publishers but also of specialists in reference works, religious books, and textbooks. These house histories, with their thumbnail sketches of colorful and idiosyncratic publishers, are gracefully written, with a keen eye for the telling anecdote, but the overall effect is inevitably one of fragmentation. The discussion of censorship, for example, is divided not only between two widely separated topical sections but also among the numerous mini-histories of individual publishers.

For all his skill and resourcefulness, Tebbel ultimately emerges as a chronicler rather than a histo-

rian. Facing “an overwhelming abundance of material” (p. xi), he simply catalogs it. Sometimes even this approach breaks down: an anecdote about the publication of the second edition of *Webster’s New International Dictionary* in 1934 is repeated almost verbatim on pages 228 and 549. Larger interpretive issues are touched on lightly or not at all. For example, Tebbel notes that Alfred A. Knopf started a luncheon group in 1927 because the older Publishers Lunch Club “was not notable in those days for the number of its Jewish members” (p. 130), but he does not explore this issue further.

Tebbel makes excellent use of *Publishers Weekly* and of the files of its editor Frederic G. Melcher and its publisher R. R. Bowker Company, but access to this rich lode of primary material seems to have deadened his broader research impulse. There are few references to interviews, company archives, collections of personal papers, or other historians’ work. Perhaps 75 percent of the citations are to *PW* or the Bowker files, an imbalance that inevitably limits the perspective. Again the treatment of censorship is illustrative: the coverage is case by case, with little discrimination between the minor and the important; no use of recent historical work on this subject; and no coherent, integrated account of the overall development in the period.

Tebbel deserves the gratitude of historians for this accurate and exhaustive compilation, and connoisseurs of publishing trivia will find his volumes endlessly absorbing (Q. Who coined the word “blurb”? A. Gelett Burgess). But the critical history of book publishing within its broader intellectual milieu remains to be written.

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JAMES N. GIGLIO. *H. M. Daugherty and the Politics of Expediency*. Kent: Kent State University Press. 1978. Pp. xii, 256. \$15.00.

If Warren G. Harding deserves a biography, and even a “failure” as president must, the man who helped put him in the White House—Harry Daugherty—merits the study that James N. Giglio has made of this sleazy politician. In fact, Giglio proposes that “there is much more to the man than his political association with Harding and his [own] troubled attorney generalship” (p. ix). Granted—though it is simply “much more” wheeling and dealing, the things he went in for from the time he first got into Ohio politics in 1881. Daugherty was twenty-one then, a small, dapper, mustachioed lawyer just graduated from the University of Michigan. Two years later he married; unlike Harding, he would never go philandering, though he was often a negligent husband and father. He practiced law

(mostly corporate), invested in real estate, lobbied (for corporations), ran for local and state office (his highest elective post was to the Ohio House), helped manage McKinley’s rise to power in the 1890s, tried and failed to carry Ohio for Taft in 1912, and in 1920 managed Harding’s run for the presidency.

After several early chapters on Ohio politics from the days of Joseph B. Foraker and Mark Hanna to those of Taft and Harding, Giglio, as he should, focuses on the Harding connection. Daugherty claimed that he thrust Harding into the Senate race in 1914. “I found him like a turtle sunning himself on a log, and I pushed him into the water” (p. 73). Giglio discounts this influence, noting that Harding was “primarily responsible for his own success in 1914” (p. 73). Harding and Daugherty enjoyed each other’s company, but “they were not bosom friends” (p. 83). Harding needed Daugherty less than Daugherty needed Harding. In 1920, Daugherty was sometimes a downright handicap to Harding in Ohio, though nationally Daugherty did make a substantial contribution to Harding’s victory. Harding, in turn, named Daugherty to his cabinet. Since Daugherty wanted the justice department, said Harding, then “by God he will be Attorney General” (p. 117). At sixty-one, at the peak of his career, Daugherty had become burly, florid, now jovial, now menacing. He set out to rehabilitate the Department of Justice, which had been “one of the most inept” branches of the Wilson administration (p. 124). It got scarcely better under Daugherty. In his best chapters, Giglio describes disasters such as Daugherty’s appointment of William J. Burns as Director of the FBI; the influence of Jesse Smith, Daugherty’s crony from Ohio, who became a “right hand man” and who presided over the “headquarters of corruption” at a little green house on K Street; and Daugherty’s mishandling of the 1922 railroad strike. By 1923, a move to impeach him was underway in the House. Harding stood by him. But then Harding died, and a Senate committee launched an investigation of Daugherty that led Coolidge to ask him to resign. Labeling his critics liars, criminals, and Communists, Daugherty went into uneasy retirement. More embarrassments lay ahead: in 1926–27, he barely escaped conviction (eleven jurors voted guilty, one for acquittal) for malodorous financial association with the Alien Property Custodian in 1921. Thereafter, Daugherty obsessively sought vindication. This he never gained. And now Giglio’s fine monograph—ample in detail, clear and judicious in narration, thorough and impressive in documentation—has surely confirmed once and for all the mediocrity of this flawed, small-town politician.

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MARTHA H. SWAIN. *Pat Harrison: The New Deal Years*. Jackson: University Press of Mississippi. 1978. Pp. ix, 316. \$15.00.

None of the fashions that now prevail in the writing of biography appears in Martha H. Swain's life of Byron Patton "Pat" Harrison, the Mississippi senator who exercised a shaping influence on the legislation of the New Deal. There is nothing here about parental conflict or, for example, the subject's developing sense of social and sexual identity, his choice of love objects, or any of the other ingredients that go into the composing of what has become known as psychobiography. There is, indeed, less measuring of the inner or private self of the subject than is usual in conventional biographies. Swain moves Harrison from birth into early manhood and into politics in approximately a page and in the remaining pages tells very little of Harrison the man. As to his personal habits, we learn only that he occasionally indulged in golfing or fishing, and, as to his domestic situation, we are told merely that he married and was the father of five children.

This, then, is a public or an official biography, dealing with the political activities of a politician. Swain does not explain why she chose this particular emphasis, but her reasons are obvious in her bibliographical essay. Almost no Harrison papers have survived, and she was forced to rely mainly on public documents, newspapers, and interviews with persons who knew Harrison. She had no choice but to take the approach that she did, and, although she was limited by her sources, she has within those limits produced a competent and useful biography, a definite contribution to the literature of the New Deal era.

Harrison served in Congress for thirty years. He began his career in 1911, assuming a seat in the House of Representatives when he was only twenty-nine years old. In 1918 he won election to the Senate, defeating James K. Vardaman, and remained in the upper house until his death in 1941 at the age of fifty-nine. During his House years Harrison supported most of the measures advocated by Woodrow Wilson, except those that he thought infringed upon states' rights, and as a senator and a loyal Democrat during the 1920s he opposed most Republican policies. Attempting to categorize him as he stood in 1933, when he emerged as one of Franklin D. Roosevelt's Senate leaders, Swain calls him a "quintessential conservative" (p. 16).

His course during the first years of the New Deal does not confirm the conservative classification. He supported most of the measures that Roosevelt proposed and, possessing great parliamentary skills, steered some of them to passage. It was evident,

however, that his heart was not always in his work, that he was actuated in part by loyalty to the president and to his party. Swain quotes a current opinion of him: "a New Deal wheelhorse . . . suspicious of his load" (p. 254). He broke with Roosevelt in 1937 on the wages and hours act and was thereafter in fairly consistent opposition. He and FDR were not reconciled until 1939 when they joined to push the defense program.

Swain does an admirable job of tracing Harrison's development from public sources. One can only wish that she might have had more private ones.

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PAUL E. MERTZ. *New Deal Policy and Southern Rural Poverty*. Baton Rouge: Louisiana State University Press. 1978. Pp. xiii, 279. \$14.95.

Following a half dozen books over the past fifteen years, this volume by Paul E. Mertz offers another critique of the New Deal's failure to eradicate rural poverty. Of all of these works, Sidney Baldwin's *Poverty and Politics* (1968) still remains the best comprehensive treatment of the subject. Using sources not previously tapped, however, Mertz has dug up new information. The freshest parts of his book concern the origins of rural relief efforts in the Federal Emergency Relief Administration, the lobbying outside Congress to pass the Bankhead-Jones bill, and the publication of the National Emergency Council's report, which called the South "the Nation's No. 1 economic problem."

Mertz asks why an administration noted for concern for the "forgotten man" could not alleviate the plight of the most destitute rural people. He divides New Deal thinking on the subject of poverty into three major patterns. The price parity program of the Agricultural Adjustment Administration became the basis of New Deal farm policy, but crop reduction drove sharecroppers off the land. The concept of rural rehabilitation—a policy of resettling poor families on land through supervised credit—originated within the FERA and was extensively used by the Resettlement Administration and the Farm Security Administration. Although reaching a half million southern families, rural rehabilitation still fell short of what was needed. The NEC report of 1938, while publicizing the region's poverty, focused attention on the need for general economic development and overlooked the most desperate segment of the southern population.

Mertz has written an able and workmanlike study, but his conclusions add nothing new. He writes that the New Deal "never found a com-



pletely effective policy for the South's rural poor" (p. 261). This failure, he thinks, was the result of congressional restraints and a lack of commitment on the part of the administration itself. The defeat of the Bankhead-Jones bill in 1935 was "the great lost opportunity of the Second Hundred Days" (p. 259). Congressman Marvin Jones did not push the bill, the legislative calendar was crowded, and Roosevelt was not solidly behind the measure. As a result, two years passed before Congress acted. The enactment of the bill, Mertz argues, would have meant the creation in 1935 of an agency like the FSA but with greater power and statutory authority. Even in 1937, the Bankhead-Jones Farm Tenant Act became law over strong opposition.

Unfortunately, Mertz fails to take his analysis one step further. He does not clearly tell us whether he thinks the New Deal should be indicted for its failure to come to grips with hard-core poverty or whether the New Deal's achievements were a credit to those who managed its programs. The challenge in New Deal history now is to weigh its record of accomplishments against the obstacles to social change. When Mertz laments the continued existence of poverty, he implies a wiser policy might have achieved better results. At the same time, he recognizes the constraints on New Deal activities. Using Mertz's evidence, one could argue that the New Deal courageously attacked chronic rural poverty and pushed a fairly effective program to its political and practical limits. In any event, students of the New Deal and agricultural history will find this book interesting reading.

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RICHARD LOWITT. *George W. Norris: The Triumph of a Progressive, 1933-1944*. Urbana: University of Illinois Press, 1978. Pp. xiii, 493. \$20.00.

With this latest book, Richard Lowitt completes his three-volume biography of George W. Norris by recounting in meticulous detail his life and work during the Franklin D. Roosevelt years. The result is a factually rich account that is unified, in one sense, by Lowitt's use of multiple perspectives for a fresh look at Roosevelt's leadership, New Deal domestic policies, and the tangled issues of peace, war, and postwar aspirations. One is able to see these large topics successively from the different but complementary vantage points of Senator Norris's politically seasoned gaze, the complicated battleground that constitutes Capitol Hill politics, and the back-home view from a portion of the nation's most desolated farm country—Nebraska. These converging perspectives provide a context in which Lowitt

deals with Norris's emergence from his long years of political opposition to a new and gratifying position of political acceptance, national influence, and personal fulfillment.

A good part of this aspect of the story deals with Norris's long struggle against the private utility interests, beginning with the fight over Muscle Shoals and ending—or starting anew—with the creation of the TVA. For years thereafter, Norris devoted a great deal of his time, energy, and parliamentary skill to securing necessary amendments for strengthening and broadening the TVA; warding off the attacks against it from opponents in Congress, the courts, and the private utilities; attempting to smooth over the damaging dissension within the TVA board itself; and protecting the independence of the agency from the jealousy and attempted encroachments of the established federal departments. Lowitt supplements this account by detailing Norris's contribution in establishing the Rural Electrification Administration, and his prolonged, ultimately successful advocacy of a series of interrelated PWA projects in Nebraska that made his home state an exemplar of public power.

Another theme that runs through this account, unifying and illuminating its detail, is the evolution of an older progressivism into the newer liberalism. Norris, who hardly can be regarded as a typical politician, nonetheless illustrates both continuities and changes in the political reformism characteristic of the Progressive and New Deal eras. Continuities are evoked by Norris's undeviating hostility to political partisanship, patronage, and party voting regularity; his untiring attack on the power trust, coupled with his commitment to publicly owned and operated power facilities; his faith in civil service, reform of governmental mechanisms, and efficiency; and his consistent support of the nation's small farmers. Change, or what can be regarded as the abandonment of nonliberal attitudes among many old Progressives, is suggestive in Norris's strong support of industrial unionism; his switch from opposition to antilynching legislation to leadership in the attempt to outlaw poll taxes; and his defense of civil rights for everyone, including the Communists. In this regard, Norris opposed the FBI's violation of civil rights and denounced J. Edgar Hoover on one occasion as "the greatest hound for publicity on the American continent today" (p. 297).

As Norris grew old in politics, he continually expanded his constituency until he became a faithful public servant not only to his fellow Nebraskans but also to the nation's farmers, workers, and urbanites. In his last years, he became increasingly preoccupied with the problems of war, peace, and postwar disarmament. Lowitt's reconstruction of Norris's slow and painful abandonment of isolation and



neutrality for internationalism and collective security provides a compelling and dramatic conclusion for a very fine biography of an exceptional American.

ALBERT U. ROMASCO  
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PHILIP J. FUNIGIELLO. *The Challenge to Urban Liberalism: Federal-City Relations during World War II*. (Twentieth-Century America Series.) Knoxville: University of Tennessee Press. 1978. Pp. xx, 273.

While there are criticisms to be made of this book, I want to begin where I shall end, stressing its value and usefulness. One can only be heartened to find historians bringing their skills to urban policy issues in numbers sufficient to give urban literature a strong base in archival research. Other disciplines have too much monopolized the story of federal urban policy and its intergovernmental aspects, especially for the 1940s and after.

This book is not about urban liberalism, nor is it an adequate account of federal-city relations, though these are clarified somewhat. Indeed, its subject is no more evident from the chapter titles than from the book title. Its subject is elusive, and I think of the book as seven somewhat related essays on federal wartime urban policies. These do not add up, any more than did federal efforts. While the author might have explained this more fully, he is hardly responsible for it.

The essays vary in importance. The first is the best, a highly valuable and concise summary of migration patterns during World War II, with special attention to the locational impacts of defense spending. A chapter on civil defense comes next, deploying an interesting tale of Eleanor Roosevelt and Fiorello LaGuardia as they toiled to make the Office of Civil Defense a place from which to (continue to, in Eleanor's case) revitalize communities from Washington. There is a chapter on defense housing and a curious one on the community services program launched by New Dealers within the National Defense Advisory Commission. Here we diverge from urbanism to programs to control VD, treat juvenile delinquency, and provide day care for children. Then we read of the National Resources Planning Board's wartime thinking about cities and of the conversation between British and American city and regional planners. Both of these chapters unearth prophetic but completely futile talk about national responsibilities for urban improvement. A final chapter traces the defeated effort to enact urban redevelopment legislation.

Philip J. Funigiello is not to be blamed if federal policies and musings did not add up to an urban

policy. Taking pieces of the story, he has illuminated them. One might have wished for a firmer grasp of evolving patterns in intergovernmental relations, but I detected no factual errors. Readers will learn much from the book, though it necessarily has a diffuse impact. It is a welcome addition to the literature on Washington's urban policies and discussions, and along the way illuminates the New Deal social engineering impulse as it fanned out into wartime bureaus and met its many defeats.

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JAMES O. FREEDMAN. *Crisis and Legitimacy: The Administrative Process and American Government*. Cambridge: Cambridge University Press. 1978. Pp. xi, 324. \$15.95.

As historians become increasingly sensitive to the significance of organizational change in twentieth century America, their awareness of the importance of administration is likely to grow. Hence this analysis of the administrative process in American government by a professor of law at the University of Pennsylvania may be of interest to scholars particularly concerned with the development of law, administration, and organizations.

Like many Americans, James O. Freedman is aware that the administrative process has raised troublesome questions about the character of American democracy. He feels that such uneasiness has engendered a sense of crisis about governmental administration in the United States. Such doubts have, in turn, impaired the legitimacy and the effectiveness of federal administrative agencies. The purpose of his book is to suggest ways and means of lessening the sense of crisis, and so to render federal administration more efficient.

In the first portion of the volume, Freedman examines the sources of the administrative crisis. He suggests that the failure of administrative agencies to conform to constitutional doctrines such as the separation of powers is one cause of the problem. Other influences that may contribute to the malaise include departures of the administrative process from judicial norms, public ambivalence toward economic regulation, and a general American skepticism about bureaucracy and administrative expertise. In addition, the public's disillusionment may have been fired by the lack of direct political accountability of most administrative agencies and by the broad powers they have exercised at the behest of Congress. Freedman feels that such criticisms have often been unfair and too sweeping.

Looking toward possible remedies, Freedman argues in the second part of the study that the most

essential source of administrative legitimacy is the quality of administrative justice. In examining formal as well as informal administrative processes he concludes—on the basis of a limited number of case studies—that the quality has been impressive. This is all the more reason, he believes, why scholars should develop an effective theory to sustain the legitimacy of administrative processes so as to counter the crisis of confidence that has been deepening for several decades.

To historians the book is not as convincing as it might be to lawyers or political scientists. Freedman has developed his themes within a narrow context of legal and administrative doctrines. He has not rooted his arguments in the rich context of historical experience. His study reflects legal formalism as distinguished from the work of scholars, such as James Willard Hurst, who place legal developments in a social framework. Although Freedman is undoubtedly correct in locating contemporary disenchantment with bureaucracy in peculiarities of the administrative process, he largely ignores the human dimension of the problem. It is possible to discuss the crisis of administrative processes meaningfully without analyzing the changing values of the individuals who operate them or the society that sustains them? A study that addresses the problem within a narrow scope may limit its appeal for a wider audience.

Historians interested in organizational behavior will want to consider Freedman's prognosis, since he deals with one of the central issues of our time.

GERALD D. NASH

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OTIS L. GRAHAM, JR. *Toward A Planned Society: From Roosevelt to Nixon*. New York: Oxford University Press. 1976. Pp. xvi, 357. Cloth \$12.95, paper \$3.95.

"Planning" in American parlance has often been a political epithet. Yet, as Charles Beard noted nearly fifty years ago, modern America cannot be understood if one ignores the development and spread of planning institutions. Early a land of engineering surveys, city planning, and corporate rationalization, it subsequently became the home of a burgeoning managerial culture constantly envisioning and seeking new planning capabilities. And, as the book under review demonstrates, post-1933 America has a substantial history of efforts to expand such capabilities into a comprehensive system of national management.

This is not to say that Otis L. Graham, Jr.'s study tells the full story. It focuses on the post-1933 presidency, not the transformation of social institutions or modern efforts to develop an integrated "plan-

ning" society as opposed to a "planning state." A more descriptive title would be "Toward a Planning Presidency." But to the knowledge and understanding of this development, Graham has much to contribute. And his work can be read with pleasure as well as profit. It is exceptionally well written, highly readable, and enlivened throughout with pithy commentary.

Graham's story begins with the planning failures of the 1930s, runs on through the frustration of planning impulses under Truman, Eisenhower, Kennedy, and Johnson, and ends with the revival of planning efforts under Nixon. Like Ernest Griffith in 1939, Graham believes that New Deal reform produced a nation more planned against than planning. The coordinating mechanism for particularistic operations became the "Keynesian Broker State," not "Liberal Planning." The latter became the "rejected New Deal"; and, once the "Broker State" was established, it became an obstacle frustrating true planning impulses. Although bits and pieces of the planning idea kept breaking through—producing things like the Paley Commission studies, the urban planning assistance program, or the National Security Council's planning board—it was not until the 1960s that pressures for "rationalizing" the New Deal heritage became strong. And not until the Nixon years did these produce efforts to design a presidency capable of evolving and implementing comprehensive growth, developmental, and social policies. The Nixon presidency, Graham finds, underwent a "more rapid evolution" toward planning capabilities than any presidency since Franklin Roosevelt's; and this, he argues, moved us substantially closer to the time when the "rejected New Deal" will become national policy.

Graham's greatest achievements are in illuminating the planning initiatives of the 1930s and of the years since 1960. His sections on the intervening period tend to be more sketchy, and his arguments concerning the future are not entirely convincing. One suspects, in particular, that he has underestimated the persisting strength of the bargaining and antiplanning traditions and the consequent likelihood that popular tolerance for national planning may be substantially less than tolerance for "stagflation" and incoherence.

One wonders, moreover, if Graham has really identified the impulses most likely to overcome the resistance to comprehensive planning. In France and Japan, the nations held up as models of workable capitalist planning, the new system has evolved not so much from a strong executive as from a fusion of corporate and governmental administration, the institutionalization of this in formalized public-private "intersects," the erection of a planning ap-

paratus on this base, and the containment of anti-planning impulses through corporative or quasi-corporative institutions. Such developments are not without their parallels in the United States, and it may be that they, rather than the quest for presidential planning tools, constitute the central story of America's move "toward a planned society."

Still, Graham has written a thoughtful and provocative introduction to the history of recent planning impulses in the United States, one that opens up largely unexplored territory and reveals a whole range of subjects about which we need to know more.

ELLIS W. HAWLEY  
*University of Iowa*

WESLEY PHILLIPS NEWTON. *The Perilous Sky: U.S. Aviation Diplomacy and Latin America, 1919-1931*. Coral Gables: University of Miami Press. 1978. Pp. 457. \$20.00.

The decade after World War I witnessed an intense competition among the United States and the European powers for the capital, trade, communications, and raw materials of Latin America. Reflecting the shift in financial power from European money markets to New York, American capitalists expanded their interests beyond the Caribbean, challenging their British, French, and German counterparts for economic pre-eminence in South America. In this study of aviation diplomacy, Wesley Phillips Newton explores part of that rivalry, recounting how American industry won the commercial battle over Latin America's skies. Initially unimpressed with the potential of airlines, neither American businessmen nor government officials were alarmed when, in the early 1920s, Europeans first offered commercial service in Latin America. Only when the German-Colombian firm, Scadta, sought landing rights in the Canal Zone did the War Department raise questions about the vulnerability of the Canal to air attack. This strategic concern combined with a new respect for international air transport inspired by the Lindbergh flight led the United States in late 1927 to formulate an aviation policy for Latin America. Seeking a corporation owned solely by United States citizens, government officials chose the fledgling Pan American Airways, providing it with diplomatic aid and the financial backing of exclusive air-mail contracts. By 1931 this official support and the business hustle of its founder, Juan Trippe, had made Pan American the dominant airline in Latin America.

As the author carefully outlines, the state department took extraordinary steps to insure that its "chosen instrument" succeeded. Though publicly

committed to the Open Door, the principle that all nations merited equality of economic opportunity, the department compromised its policy in promoting Pan American. It consistently favored the airline over competing American interests and acquiesced in Trippe's desires for "essentially exclusive, preferential, and monopolistic concessions" yet protested whenever a Latin country contemplated granting similar privileges to an airline owned by Europeans. Indiscriminate competition among American companies could not be allowed to jeopardize the strategic and commercial objectives of the United States in Latin America.

Newton's approach to the subject is cautious and sober, and his account is based on extensive research in archival and published materials. But his focus is narrow. He fails to engage in the debate among historians, such as Michael Hogan, Emily Rosenberg, Joseph Tulchin, and Joan Hoff Wilson, on the nature of United States foreign economic policy in Latin America. Indeed, he draws no conclusions on how his work has enhanced scholarship on the patterns of United States foreign policy except to mention briefly that aviation diplomacy was part of both Dollar Diplomacy and the Good Neighbor. The author has given us a narrative, not a critical, analytic study.

STEPHEN G. RABE  
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Dallas*

WILLIAM J. SCHMIDT. *Architect of Unity: A Biography of Samuel McCrea Cavert*. New York: Friendship Press. 1978. Pp. vi, 330. Cloth \$14.95, paper \$9.95.

Drawing deeply upon the records and publications of the federal, national, and world councils of churches and upon his own collaboration with Samuel McCrea Cavert in writing two volumes on ecumenical history, William J. Schmidt has produced a richly detailed biography of the man whom he considers "the American ecumenist of the twentieth century" (p. 313). Although not uncritical, the work is openly affectionate; Schmidt calls Cavert "my chief mentor in things ecumenical" (p. i).

This biography meticulously traces Cavert's activities as an ecumenical executive: director of the Student Y.M.C.A. at Union College, general secretary for nearly thirty years of the federal council, an important figure in the creation of the world council and for a time head of its New York office, first general secretary of the national council, and the holder of numerous other positions. It appears that no other American has had as long a career of ecumenical leadership. The ecumenical movement occupied virtually his whole life. For that reason,

and perhaps also because of Cavert's personal reserve, the account is heavily organizational. After the first two chapters, which treat his lineage, childhood, education, and early adulthood, there are few glimpses of his family life or other interests he may have had. Because Cavert kept himself largely out of his own historical narratives, however, this reconstruction of his organizational role has considerable value.

The evolution of Cavert's thought receives competent, if not always systematic, analysis. Schmidt portrays him as an evangelical liberal whose inherited Presbyterian orthodoxy was broadened by the Social Gospel, which in turn was modified in the 1930s by neo-orthodoxy and a growing appreciation of the significance of theology. An emphasis on "the Church and its quintessential functions" replaced preoccupation with establishing the Kingdom of God on earth (p. 108). Despite changes in emphasis, however, Cavert always insisted on conjoining the personal gospel of individual regeneration with the imperative of social amelioration. He shared Protestant liberalism's commitments to racial and economic justice and to the cause of world peace.

Cavert's enhanced view of the nature of the church led him to interpret the ecumenical movement as "not an extra-ecclesiastical thing" (p. 119). Rather, he saw in ecumenism a pragmatic means of transcending denominationalism while representing denominations. Organic union, though the ultimate ideal, would not come quickly. Meanwhile, the conciliar ideal, "both method and paradigm" for achieving Christian unity, provided a framework that expressed freedom, diversity, and oneness (p. 310). Through the many vicissitudes of ecumenical history, he remained convinced of the necessity of Christian unity and hopeful for its growth.

A lifelong bureaucrat, Cavert practiced a style of leadership that was low-keyed and harmonizing, granting his staff considerable autonomy, rejoicing when his objectives were reached even if he did not receive the credit he deserved, and often mediating between the more aggressive personalities in the ecumenical movement.

Though Schmidt's writing is not sprightly and the organizational details sometimes tax the reader's interest, the work is a worthy addition to the neglected field of ecumenical history.

JACOB H. DORN  
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MORRIS JANOWITZ. *The Last Half-Century: Societal Change and Politics in America*. Chicago: University of Chicago Press. 1978. Pp. xiii, 582. \$25.00.

This volume is an ambitious attempt to synthesize certain "master trends" of the period 1920-76 into

an overall account of social control in the United States, considered as an example of an advanced industrial society. These trends include the decline in citizen identification with political parties and the development of politically weakened national regimes, the growth in the participation of government in economic distribution processes, and a fragmentation of social strata into differentiated (rather than strictly stratified) groups based on occupation, skill, profession, and capacity to claim transfer payments in the economic dimension as well as on sexual, regional, and ethnic differences. These trends are also held to interact in such a way as to reinforce one another, social segmentation rendering the calculation of political self-interest more problematic and promoting political instability and disaggregation. The professionalization of military service, the advent of "deterrence" as a feature of modern military strategy, and the dismantling of the concept of the citizen army of World War II vintage, particular scholarly concerns of the author, are likewise considered to have had the effect of weakening the capacity of the government to mobilize majority sentiment in behalf of its international goals, especially limited wars.

Propositions such as these are assembled to trace contemporary patterns of social control in four institutional contexts: the world of work, with the growth of bureaucratic organizations; residential communities, ever more specialized and physically removed from work sites; media of mass persuasion, with their potential for the creation of popular cultures and for society-wide socialization; and more direct socialization processes by means of legitimate coercion. Emphasis here is likewise laid on the cumulative effects of "disarticulation," increasing social distance among previously well-connected actors resulting in new social strains.

In a final section, Morris Janowitz assesses the possibilities of building social institutions capable of managing these strains. Thus, despite the backward-looking promise of the title, this is in fact a forward-looking book. Developmental sequences are traced primarily for the purpose of understanding their dynamics well enough to discover principles of guidance and amelioration.

Just as this is very much a social scientist's rather than a historian's look at history, so also is it very much a book that is explicitly rooted in the Chicago style of social science, drawing upon the activist tradition of Robert Park, Louis Wirth, and Charles Merriam, especially as elaborated by the late Harold Lasswell into the notion of policy science. Grand synthesis on large historical themes, Janowitz reminds us, is not the exclusive prerogative of European polymaths or epigones of one or another global thinker of the late nineteenth century. Both



the texture of Janowitz's argument and the vocabulary in which it is expressed may prove unfamiliar to many historians and not altogether congenial. Those likely to be least put off will be historians who are sympathetic to the mining of the past for lessons to be applied to future conduct and who are undaunted by the eclectic spirit that informs this book.

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RICHARD B. CALHOUN. *In Search of the New Old: Redefining Old Age in America, 1945-1970*. New York: Elsevier North-Holland. 1978. Pp. v, 280. \$19.95.

The "new old" referred to in the title of this book are Americans born around 1900—the "flaming youth" of the 1920s, "senior citizens" or "golden agers" in the 1960s, Gray Panthers in the 1970s. Those alive in 1970 had spent the latter half of their lives after passage of the Social Security Act; in addition to federal old-age insurance, many of them had private retirement plans obtained through collective bargaining or voluntarily granted by employers. Theirs was the first generation to reap the benefits of the modern American welfare state, the first to face old age without the fear of poverty that had harassed most of their forebears.

Although these people are the real subject of the book, they appear only briefly, as successful lobbyists for increased social security benefits, Medicare, and Medicaid. For the most part their "image," not their actuality, is what interests the author. The study, which was financed by the Administration on Aging of the Department of Health, Education, and Welfare and produced by the Center for Policy Research, New York City, deals with changing attitudes toward old age and the elderly between 1945 and 1970. The thesis is that gerontologists, social workers, educators, writers and broadcasters, advertisers, and businessmen all contributed, though for different reasons, to a more positive view of old age and a distinct improvement in the status of the aged.

Another interpretation would be that the more genial view of the elderly in literature, scholarship, and marketing simply reflected underlying improvement in the social and economic condition of many old people. As a study of "changes in society's conceptions and expectations of the elderly during the past twenty-five years" (pp. 18-19), the book is dated because so many changes have occurred in American society and the situation of the aged since 1970. As a study of social change in the period 1945-70, it seems out of focus because of failure to take into consideration significant shifts in attitudes

toward old age that had occurred before 1945 and the sketchy, unconvincing account of the treatment of older people in print media, 1945-50.

Aside from these objections and read just as a broad-ranging survey of opinion about aging and the old in the United States from the end of World War II to the mid-1960s, the book has much to recommend it. The author has assembled, organized, and conscientiously and appreciatively analyzed a wide variety of both scholarly and popular writings that sought to understand, explain, and appeal to older people in America's short-lived age of affluence. The chapters on contributions of social scientists and marketers to images of the aged and on retirement programs and policies are particularly interesting and informative.

ROBERT H. BREMNER  
Ohio State University

RICH JOHNSON. *The Central Arizona Project, 1918-1968*. Tucson: University of Arizona Press. 1977. Pp. vi, 242. Cloth \$11.50, paper \$5.95.

This book is an insider's view of the tangled political machinations that culminated in 1968 in congressional approval of the Central Arizona Project (CAP), a billion-dollar engineering feat designed to transport a large portion of the Colorado River to the water-shy heartland of Arizona. The account reinforces the critical importance of water in the political and economic life of the arid West and provides additional insight into the bitter and often highly complicated intra- and interstate rivalries over the precious commodity.

Rich Johnson became a participant in many of the events he describes while serving as a reporter covering the pre-trial hearings in the *Arizona v. California* case in 1955. His involvement increased the following year when he became executive secretary (and eventually president) of the Central Arizona Project Association, the principal lobby for the CAP, and intensified further in 1965 when he became executive director of the Arizona Interstate Stream Commission, the state's official watchdog over the Colorado River. These vantage points have enabled Johnson to provide new information about disputes among Arizonans over the state's policy toward California during the 1950s and 1960s and over whether Arizona should build the CAP itself or continue efforts to persuade the federal government to construct it; about the transformation of Arizona and California from being archenemies to joint advocates of the CAP; and about off-the-record maneuvers of the seven Colorado River states which helped shape the 1968 legislation that authorized the project.



Johnson was (and is) an unabashed enthusiast of the CAP, so readers will not find here an objective or complete appraisal of the arguments marshaled against the project by environmentalists and others. Still, he does not ignore the opposition and his account is considerably more balanced than John U. Terrell's *War for the Colorado*, another firsthand statement written from a California (and anti-CAP) perspective.

When read as something other than a memoir, the book is less valuable. The bibliography is disappointingly brief; it consists mostly of published congressional hearings, contains only one item published after 1969, and draws on no unpublished papers (except apparently the author's own notes). This helps explain some of the book's shortcomings: the failure to note nineteenth-century progenitors of the CAP; the overemphasis on Fred Colter and neglect of George Maxwell and Governor George W. P. Hunt in explaining Arizona's long opposition to the Colorado River Compact; the uncritical acceptance of the U.S. Supreme Court's interpretation of the Boulder Canyon Project Act in its *Arizona v. California* decision; and the occasional tendency to view Arizona's position in that case as being identical to the court's decision. Also disappointing was the author's decision to eschew the use of footnotes.

But these reservations do not detract from the book's value as a firsthand account of the struggle for the CAP between 1955 and 1968. I hope that Johnson will someday provide a sequel, for the fight for the project is not over. Though Congress authorized the CAP in 1968, construction did not begin until 1973 and opponents are attempting to prevent or stall congressional appropriations needed to complete the project.

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STEPHEN D. KRASNER. *Defending the National Interest: Raw Materials Investments and U.S. Foreign Policy*. Princeton: Princeton University Press. 1978. Pp. xiii, 404. Cloth \$20.00, paper \$5.95.

Stephen D. Krasner portrays the state as an autonomous actor pursuing the "national interest," by which he means the "preferences" of "central decision-makers" in the White House, the State Department, and other "central-state institutions." Concerning raw materials policy, he identifies these preferences as furthering certain broad political and ideological objectives, ensuring security of supply, and increasing economic competition. Krasner explores fifteen case studies ranging from petroleum and rubber policies in the 1920s, through the nationalization of American-owned properties abroad

between 1910 and the present, to the oil crisis of the 1970s. These explorations, he argues, prove the superiority of a "statist" paradigm over either a liberal pluralist or Marxist interpretation of American raw materials policy. Marxists, he contends, incorrectly deny the independent causative power of ideology, and both Marxists and liberal pluralists wrongly view state action as a byproduct of pressure from particular societal groups or classes. Krasner admits that the diffusion of power in American society can frustrate efforts by policy makers to implement their preferences over the objections of specific societal groups. Yet they can do so, he insists, by exploiting group divisions and limiting the decision-making arena to central-state institutions where special interests exert little influence.

Although timely and provocative, this study is seriously flawed. Krasner never clearly defines such key concepts as "central decision-makers" and "central-state institutions." His effort to disprove the Marxist contention that ideology is not an independent causative factor leads to an artificial separation of ideological-political and economic policy objectives. He might be correct in characterizing the American political system as "weak" because public officials often have to bargain with private interests to achieve policy objectives. Yet he would have told us more about the American system, and American ideology, if he had explained why public and private leaders prefer bargaining, voluntarism, and public-private cooperation to coercion.

Most importantly, Krasner stakes his case against the structural Marxist interpretation on two unsuccessful arguments. The government's proposal to purchase Aramco's Middle Eastern oil concessions in 1943, he argues first, reveals the readiness of independent policy makers to violate a fundamental precept of capitalism—the private ownership of the means of production. Yet, the plan contemplated called for a joint government-business project in which Aramco would own two-thirds of the enterprise. Such a scheme hardly undermines the principle of private ownership.

According to Krasner's second argument, decision makers sometimes misperceived American interests and adopted policies that undermined domestic stability. Such behavior supposedly disproves the contention of structural Marxists that the state must always act rationally to preserve the coherence of the capitalist system. The difficulty with this argument is Krasner's confessed inability to prove its applicability to the area of resource policy. Near the end of the book he admits that, if the cases then under review presented "all the evidence that could be brought to bear to distinguish between a statist and structural Marxist interpretation, it would not be possible to give a decisive

defense of either proposition" (p. 320). Yet he does not have another case to offer. Instead, he gives a brief account of American irrationalities in Vietnam, acknowledges that it takes "us rather far afield" (p. 325) from the subject of his book, but asks the reader to assume that it proves the superiority of his statist paradigm in the area of raw materials policy! With such arguments, Krasner admits defeat for his interpretation.

MICHAEL J. HOGAN  
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PEER DE SILVA. *Sub Rosa: The CIA and the Uses of Intelligence*. New York: Times Books. 1978. Pp. xi, 308. \$12.50.

One consequence of the evaporation of the Cold War consensus in the late 1960s has been a spate of books on United States secret intelligence operations. Most such works are polemical, usually aiming to defend, but occasionally to condemn, American secret intelligence and foreign covert actions over the past thirty years.

Peer De Silva's motive clearly is to defend the Central Intelligence Agency, which he sees as "caught up in a deadly struggle with its natural enemy," the KGB (the Soviet CIA), "an aggressive, amoral and implacable foe of the American government and the American people" (pp. x-xi). This quotation portrays the author's mind-set with regard to the Soviet Union, the CIA's main target and preoccupation for the past thirty years. But his principal purpose is to provide a memoir of twenty-eight years as a professional "in the field of espionage and counterespionage." He offers this book, presumably with a proper CIA clearance, to fulfill a need, as he expresses it, "for a correct and honest picture of how the CIA does, in fact, work and operate abroad and at home" (p. ix). He asserts that critics of the CIA "in academe and the media" have engaged in a "crucifixion" of the CIA. He proposes to set the record straight.

The autobiographical chronology begins with the author's service in Army intelligence in the late 1940s. He became a civilian CIA officer in 1953 and then served as the CIA's operations chief for Soviet activities for more than three years. Details of his world travels and the minor espionage adventures involved offer little new information that would enable historians to evaluate the quality of the CIA's work during the period. In the book's introduction, the author makes the implicitly promising reference to the "terrible failures" and "magnificent successes" of secret operations. From the evidence offered, however, one is hard put to find examples of success, or, indeed, to understand the many intelligence failures.

The most revealing portions of this book deal with the author's assignments to the Far East, first to Korea in 1960 and later to Saigon as chief of station. Here one has glimpses into the bureaucratic politics and maneuvering in the field among the CIA, the armed services, and the Department of State. The latter part of the book is devoted to aspects of the early stages of the American intervention in South Vietnam. He argues against the attempt by the United States at a military "solution" in Southeast Asia. His argument is that the CIA had developed political solutions to the Vietnam puzzle but lost out in the policy fights with the American military. The author left Vietnam in 1965 after a serious injury from a bomb. His final years in the CIA, from 1966 to 1973, were uneventful.

Like most writing on this subject, this book compounds the conceptual confusions that abound regarding "intelligence" work. De Silva makes no effort to clarify the distinctions among such disparate functions as "intelligence," "counterintelligence," "espionage," "psychological warfare," "covert action," and "para-military operations." The book's main value is the insight offered into the mind-set, values, assumptions and life style of an American intelligence professional serving overseas. When added to many other similar works, we may some day better understand the secret side of thirty years of Cold War. And we may be able better to comprehend the uses, and the abuses, of secret intelligence that were significant hidden factors in American foreign operations.

HARRY HOWE RANSOM  
Vanderbilt University

ROBERT JUSTIN GOLDSTEIN. *Political Repression in Modern America: From 1870 to the Present*. Boston: G. K. Hall. 1978. Pp. xxii, 682. \$22.50.

Political repression, Robert Justin Goldstein argues, has not been given its due as a factor in the formation of the modern American polity. His extended survey of repressive policies and activities over the past century is a serious attempt to remedy the deficiency, and, at a minimum, his work ought to provoke more intensive investigation of the subject.

Goldstein's principal contribution to understanding the problem is his demonstration that repression has been more nearly periodic than episodic, a point that the shifting character of persecutors and persecuted has obscured. Powerful private interests and their protectors in state and local government took the lead in initiating repression, at least until World War I; their preferred target was organized labor in its more radical forms. In more recent decades, by contrast, specifically political dissidence

has been the designated enemy, with the federal government assuming the role of Grand Inquisitor.

The author's attempt to construct a model for repression, however, leads to oversimplified explanation. Although a decision on the part of political authorities to carry out repressive policies seems self-evident as an invariable condition of the problem, the variable conditions and motives for repression posed by Goldstein tend to be incomplete, to say the least. His attempt to absolve "the people" of responsibility for bouts of repression by insisting on their passivity results from failure to ask enough questions of the evidence.

In dealing with the early Cold War period, for example, Goldstein accepts polls indicating relatively low public concern with the Communist "menace" as proof that public opinion was not a positive element in the explosion of McCarthyism. Yet he also cites polls indicating a continuous and dramatic decline in the toleration for dissent. What Goldstein overlooks is the possibility that anti-communism was thrown as a cloak of respectability over assaults on other groups. Midwestern and southern resentment of eastern dominance of the governmental and business establishments and the counterattack of some religious and ethnic elements against former persecutors joined hands with Negrophobia and anti-Semitism to form parallel constituencies for repression. Their *real* concern was not communism, but their targets had to be identified, however tenuously, with communism to give their hostility an air of patriotism.

Goldstein does recognize the sinister significance of the Nixon repression, carried out largely undercover, since it lacked the support, and sometimes proceeded against the opposed opinion, of key elites and the public. The operative decision was made by the central authority and was not contingent upon wider approval. Nixon's is also the most unambiguous example of repressing an opposition just because it was an opposition.

Where the author's analysis lacks depth, it may be the victim of his method. The scope of his subject, he tells us, compelled him to rely exclusively on secondary material for the period 1870-1960, and it is precisely for these years that Goldstein's analysis appears too simple. Since it does not appear that he had previously done primary research on any particular phase of the question, and since he excludes personal accounts and most autobiographical material on grounds of their self-serving quality, it is not clear what, except perhaps his own political inclinations, has guided him in making the necessary choices among his sources. Goldstein, nonetheless, has seen through a formidable task and laid down a challenge to future research in the field.

It must be said, to conclude *diminuendo*, that the book as produced bears numerous marks of indiffer-

ent editing and casual proofreading. The author deserved better.

ALAN D. HARPER  
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MILTON CANTOR. *The Divided Left: American Radicalism, 1900-1975*. (American Century Series.) New York: Hill and Wang. 1978. Pp. 248. Cloth \$11.95, paper \$5.95.

In the introduction to *The Divided Left: American Radicalism, 1900-1975*, Milton Cantor asks: "why didn't criticism of capitalism [in America] transcend the capitalist structure and turn toward an alternate economic order?" The question is a good one; indeed, it is the essential one to answer if we wish to understand the fate of radicalism in twentieth-century America. Alone among the industrial nations of the world the United States failed to develop a class-conscious proletariat and a strong socialist party based on workers' sense of solidarity and their opposition to the men of property. Why?

Cantor attempts an answer in his introductory pages. He relies on a version of the "false consciousness" notion as expounded by the Italian Marxist theoretician, Antonio Gramsci. Within capitalist society the values of the capitalists become supreme—"hegemonic" is Gramsci's term—and even those objectively exploited by the capitalist system come to accept its values, goals, and ways of thinking. People so affected do not confront capitalism. They struggle within it to seize its benefits and blame only themselves when they fail to attain them.

No matter how subtly, or in how sophisticated a way, this argument is presented, it amounts to saying that the power of the hegemonic culture is so great that it can overcome the reality of workers' lives and convince them that dark is light and bad is good. Perhaps this is so, but as an explanation for socialist failure it suffers from two serious flaws that must give socialists particular pause: it gives culture priority over "the material relations of production," and it suggests that workers are not as capable as intellectuals of seeing the answers to the problems that beset them. The first of these reverses the usual order of Marxist analysis; the second is implicitly elitist.

Despite these objections, if Cantor's brief survey of the American left during the twentieth century had served to demonstrate his conclusion, it would have been interesting. It does not. Cantor announces his thesis on pages three through ten and then, for the remaining 220 pages of his work, says little more about it. The rest of the book is a careful, well-organized, conventional review of the American left from Daniel DeLeon's Socialist Labor Party

of the 1890s to "the radical scene in the seventies." There are chapters on the cultural left, but much of the text is organized around major socialist organizations including the Socialist Party of America, the IWW, and the Communists. We are shown in detail the factional divisions and the changing responses of each group and are introduced to the dramatis personae of American radicalism.

Cantor's story has been told many times before and one wonders why we need another survey of this kind. The editors undoubtedly believed that a compact volume covering the whole twentieth-century American left would find a place in undergraduate courses. Unfortunately, there are already two recent works, one by John Diggins and one by James Weinstein, that are equally brief and better. If I were seeking such a book I would adopt one of these. Nevertheless, *The Divided Left* is a respectable alternative.

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HOWELL RAINES. *My Soul Is Rested: Movement Days in the Deep South Remembered*. Reprint. New York: Bantam Books. 1978. Pp. viii, 540. Paper \$2.95.

Periodically there appears a book of such compelling historical and social value that we cannot ignore it. Howell Raines has produced that kind of volume. Using his journalist's sense of significance and detail and employing the techniques of oral historians to good effect, Raines interviewed over six dozen participants in the southern civil rights struggle of the 1950s and 1960s. These discussions bring to life an era that underlies much of what America has subsequently sought to become.

Those who shared, however minimally, in the incidents depicted will find bittersweet affirmations of their past. Students too young to remember Albany, Georgia, and Selma, Alabama, but wholly impatient with the impersonal forces afflicting their lives, need to know how Lonnie King, Connie Curry, Ivanhoe Donaldson, and hundreds of others interrupted their plans and redirected their energies in an urgent drive to establish justice in communities seldom touched by equity or compassion. Groping for purposefulness at a time of drift and fragmentation, today's middle-aged citizens, especially faculty members, should review these accounts of a campaign that transformed America from its complicity with southern segregation to its 1970s quest for nationally sustained affirmative action.

The scope of Raines's interviews is impressive. He makes us privy to the distant motives and actions of movement elders (James Farmer, Rosa Parks, Fred L. Shuttlesworth, Bayard Rustin, Ruby Hurley,

Ralph Abernathy, among others) and younger participants, many of whom now occupy positions of public trust and visibility (Andrew Young, Marion Barry, Julian Bond, John Lewis, for example). At the grass-roots level, he captures the recollections of men and women who daily faced the most severe segregationist reprisals but never flinched in their quiet determination to invoke change. Sections entitled "Lawyers and Lawmen" and "Reporters" put us in touch with national, state, and local personalities forced to respond to the creative initiatives of civil rights activists. Among the most fascinating conversations are those with law officers and white community leaders, who disclose, some for the first time publicly, the informational and associational networks on which they relied for dealing simultaneously with the White House, the governor's office, local officials, and civil rights personnel. Interviews with die-hard segregationists like Robert Patterson of the Citizens Council, Robert Shelton of the Ku Klux Klan, and J. B. Stoner of the National States Rights Party indicate that racism still thrives. Overall, the book reveals how fragile a commodity social justice really is.

Raines is careful to explain his methodology and that earns him a full measure of the readers' confidence. He deserves appreciation, too, for uncovering individuals who are ten, twenty, and more years removed from the public's line of vision and for arranging his materials (gathered between October 1974 and April 1976) in coherent and systematic fashion. Historians will especially like his unobtrusive headnote and footnote commentaries, which provide updated information about the persons and groups discussed. There remains one regret: that Raines did not interview a more balanced share of women participants in the crucial events that are otherwise so faithfully depicted.

ROBERT L. ZANGRANDO  
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FRANK T. READ and LUCY S. MCGOUGH. *Let Them Be Judged: The Judicial Integration of the Deep South*. Metuchen, N.J.: Scarecrow Press. 1978. Pp. xiv, 658. \$22.50.

In this absorbing study of an appellate court's influence, the authors explain that the Supreme Court's 1954 decision in *Brown v. Board of Education* did not integrate the South's public schools. The real task of integration was handled by lower court federal judges in following the Supreme Court's instructions in the 1955 second *Brown* decision. The lower courts were successful in their efforts, and, despite verbal opposition, massive resistance, and violence, deep South public schools became more in-



tegrated than those in the North. The United States Court of Appeals for the Fifth Circuit, which had responsibility for the supervision of all court activity in Texas, Louisiana, Mississippi, Alabama, Georgia, and Florida, forged the evolving law of desegregation in the late 1950s and the 1960s. This study of the Fifth Circuit's contributions is a welcome addition to the literature on law and civil rights and reminds us of the difficulties in trying to follow the southern example in the North.

One highlight of the book is a rather detailed examination of the interference of HEW, under the leadership of Secretary Robert Finch at the instigation of Richard Nixon and John Ehrlichman, with the desegregation process ordered by the Fifth Circuit Court. The Finch intervention in the form of proposing a delay in desegregation in the name of Nixon's southern strategy backfired. After a hoped-for delay was announced, the Supreme Court overruled the Fifth Circuit and ordered more, rather than less, immediate desegregation.

In remarkable detail, the authors discuss the shift in the Supreme Court from the "all deliberate speed" injunction in *Brown* to the insistence that school desegregation must be immediate in the late 1960s. Frank T. Read and Lucy S. McGough believe that most whites in the South have accepted the integration of their public schools and that the question remaining for the country is whether integration will occur in the North and West. They believe that the Supreme Court decision against a metropolitan-wide remedy in the Detroit case, *Miliken v. Bradley*, will be an effective deterrent to integration in the northern ghettos and the western barrios. The Detroit case, they believe, may mark the end of the Second Reconstruction just as the Compromise of 1877 marked the end of the first Reconstruction. They explain, however, that the Detroit case may be seen as a simple recognition that something more than a legal solution is needed to provide an equal opportunity for education to all Americans. They believe that the division among blacks themselves, as reflected in the arguments made in the *Atlanta* case over whether it is important to desegregate the schools or to have black control of the schools even though they remain segregated and in the controversy about busing, make the problem even more difficult. The authors point out that, "as long as blackness and poverty are inescapably linked, and as long as minority plaintiffs cannot themselves agree on the proper remedy, the 20-year effort to implement the promise of *Brown* may have in fact reached its logical conclusion." Perhaps, they suggest, the problem of school segregation in cities cannot be solved without new tools that address the problems of poverty to augment the legal theory stated in *Brown*.

This book is both a history of the Fifth Circuit and a constitutional history of school desegregation. It also goes beyond the scope of public school desegregation to discuss the Supreme Court decisions in race relations cases involving higher education, voting rights, jury selection, and public accommodations. It is a model study of decision making in an intermediate court and a welcome addition to the large body of literature on the Supreme Court's race relations decisions. It is comprehensive, detailed, and based, apparently, on a wide variety of primary and secondary sources. One defect in the manuscript, for the scholar, is that footnotes are absent. There are summaries of part of the material on which a chapter is based at the end of each chapter, and the introduction indicates that copies of the original manuscript containing exhaustive footnoting are available upon request.

The authors fully considered the legal problems of desegregation, the history of the court, and the background of the judges, but the reader could use more information concerning what actually happened to the children when the schools desegregated. Otherwise, desegregation may appear to be more successful than it was. For example, some attention to suspensions from the schools, black and white student achievement gains and losses, and the displacement of black teachers and black administrators in the process would go a long way toward explaining why some blacks and whites have abandoned the effort to finish the task of school desegregation. None of these criticisms should be taken to detract from the overall importance of the authors' accomplishment.

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ROBERT A. DIVINE. *Blowing on the Wind: The Nuclear Test Ban Debate, 1954-1960*. New York: Oxford University Press. 1978. Pp. ix, 393. \$14.95.

This book represents an important addition to popular and scholarly knowledge, for it deals with one of the most vital issues of the twentieth century: the American response to nuclear weapons. During the 1950s, Robert A. Divine argues, "the advent of the hydrogen bomb and the attendant possibility of global destruction" posed a problem that "almost no one confronted openly." Instead, Americans "focused on the more immediate and less terrifying issue of fallout from nuclear tests" (p. viii). Thanks to the popular clamor, at home and abroad, the Eisenhower administration gradually shifted its position on nuclear testing from providing unquestioning support to securing a negotiated test ban. The author maintains that, even though problems of de-



tection and the U-2 incident soon halted this dovish advance, Eisenhower, Stassen, the White House scientific advisers, and, to a lesser extent, Dulles and Herter, deserve considerable credit for overriding the opposition of the Atomic Energy Commission and the Pentagon to launch a moratorium on testing and establish the preconditions for the test ban treaty of 1963.

Well written, detailed, and thoughtful, the book provides us with much new information and insight. One discovers, for example, that AEC chairman Lewis Strauss insisted that the *Lucky Dragon*, a Japanese fishing boat showered with U.S. nuclear fallout, was actually "a Red spy outfit" (p. 11); that White House aides forwarded postcards critical of U.S. nuclear tests to the FBI for investigation; that zealous advocates of developing a "clean" bomb were somewhat deflated by Eisenhower's logical suggestion that they give it to the Russians; and that polls reported college-educated Americans far less likely than those with only a grade school education to favor a suspension of nuclear testing. The author shows particular skill in elucidating the conflicting claims among scientists about the dangers of fallout and the possibilities of detecting nuclear tests. Most important, scholars can learn more than ever before about the handling of nuclear weapons testing by the Eisenhower administration.

Yet the overarching thesis of *Blowing on the Wind* is open to serious question. By arguing that the test ban represented "a kind of magic talisman"—an irrational escape from "coming to grips with . . . the possibility of total destruction" (p. 323)—the author tends to distort the meaning of much opposition to nuclear testing. After all, both the American military and its critics well understood that a halt to H-bomb tests would seriously impede the arms race. In 1956, when Adlai Stevenson called for the suspension of nuclear testing, it was for the purpose of checking "this policy of trying to preserve the peace by a preponderance of terror" (p. 106); letters praising his stand poured into his campaign headquarters at the rate of one thousand a day. Undoubtedly, some Americans (including those in the government) did not (or would not) recognize that a nuclear war left them no escape, but many were under no illusions. Indeed, the author cites a Gallup poll as early as April 1954 in which 57 percent of respondents claimed that there was a fair to good chance that their city would be attacked with a nuclear bomb. Furthermore, the fallout from nuclear testing was biologically dangerous and, therefore, represented a justifiable grievance of the average citizen against the arms race. Should peace groups be portrayed as irrational for pressing it as an issue? And, if so, what is one to conclude about the federal government, methodically preparing the nation for nuclear war?

A more serious problem of the book lies in the unavoidably limited scope of its research. Although the handling of the public debate on nuclear testing is solidly grounded in the relevant sources, restrictions on access to U.S. government records seriously hamper the treatment of government policy making. The author has made good use of the Eisenhower papers, but has been able to examine only a few of the items from the AEC records and none of the materials from the files of the Department of State, the National Security Council, and the Department of Defense. At key junctures, he is compelled to fall back upon public speeches, news stories, magazine articles, and personal speculation to explain the motives and tactics of the administration. Consequently, while *Blowing on the Wind* makes a major contribution to our knowledge of the public and private debate about nuclear weapons testing, a complete understanding of government policy awaits scholarly access to those public records that still remain behind closed doors.

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LAWRENCE M. BASKIR and WILLIAM A. STRAUSS. *Chance and Circumstance: The Draft, the War, and the Vietnam Generation*. New York: Vintage Books. 1978. Pp. xix, 312. Paper \$3.95.

Of the nearly twenty-seven million young men of draft age during the Vietnam War, the most despised and least understood were those who either resisted the draft or deserted from military service. In the popular imagination, these young men were affluent, well-educated, political radicals who, out of either cowardice or conscience, refused their obligation to defend their country. Lawrence M. Baskir and William A. Strauss, members of President Ford's Clemency Board, drawing upon data developed by that body, correct this mistaken impression and in this excellent study demonstrate the larger failure of the draft during the Vietnam period.

In *Chance and Circumstance* Baskir and Strauss argue that, far from being equitable or fair, the draft was an "instrument of Darwinian social policy" (p. 6) in which those with money and education escaped while the poor and uneducated did the fighting and dying in Vietnam. They conclude that by 1971 "the enforcement of the draft law was in a shambles" (p. 68) because local draft boards adopted different standards for awarding deferments, for granting conscientious objector status, and even for prosecuting those who violated the draft laws.

Not surprisingly, the authors show that resisters and deserters came from the same group of poor and uneducated that bore the brunt of the war. The Clemency Board found in one sample of eighteen hundred draft offenders that the vast majority were poor and uneducated, and the board recommended pardons in four out of five cases. On desertion, the authors conclude that during the war "politically motivated deserters were far outnumbered by those whose absences were not direct responses to their feelings about the war" (p. 109). They explain the disparity between their findings and the popular image of resisters and deserters by asserting that these young men became whipping boys for America's failure in Vietnam: "They are the ones to blame for the tragedy of a lost war and lost illusions, symbols of the nation's lack of resolve to win" (p. 11).

While the failure of the draft can be explained by bureaucratic failures, Baskir and Strauss charge that Project 100,000—Lyndon Johnson's plan to use military training and discipline to rehabilitate the disadvantaged youth—was a cynical, self-serving, political move. Project 100,000 allowed Johnson to fight the war without mobilizing the reserves or ending deferments for the children of the more politically sensitive middle class. Since roughly half of the quarter of a million Project 100,000 men were "rehabilitated" by being sent to Vietnam, it is difficult to dispute their claim. *Chance and Circumstance* also shows that both the Ford clemency program and President Carter's "blanket pardon" program were ineffectual. In the former program only 6 percent of the three hundred and fifty thousand eligible resisters ever bothered to apply, while in the Carter program only ninety-nine sought the pardons available to two hundred and sixty-five thousand resisters.

Finally, Baskir and Strauss fear that the draft experience of the Vietnam era will cast a long, dark shadow. The Vietnam generation—that generation of avoiders, evaders, resisters, deserters, and disillusioned veterans, deeply embittered by their experience—is likely to transmit its bitterness to future generations. And that, the authors assert with understatement, "is not the stuff upon which to build an ethic of unquestioning patriotism" (p. 243).

*Chance and Circumstance* is an important book that explicates a widely suspected but little appreciated aspect of the Vietnam tragedy. By the 1960s American society had come to accept the notion of involuntary military service as a necessary evil. It fell to members of the Vietnam generation, and their parents and relatives, to discover the gross inequities of that system. A full understanding of the dimensions of that failure is essential today as American society debates anew the relative merits of a volun-

teer force or the reinstatement of some form of involuntary service.

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ALLAN E. GOODMAN. *The Lost Peace: America's Search for a Negotiated Settlement of the Vietnam War*. (Hoover Institution Publication Series, number 173.) Stanford, Calif.: Hoover Institution Press. 1978. Pp. xviii, 298. \$22.50.

*The Lost Peace* is a concise summary of negotiations during the Vietnam War. It is based almost entirely on "off-the-record" interviews with decision makers in Washington and Saigon and tells the story of "what happened and why from their perspective" (p. 1). North Vietnamese leaders refused Allan E. Goodman interviews and he relies on published material for Hanoi's actions. He does not attempt to examine all the material on the Vietnam War but rather concentrates on the negotiation process and its effect on the war.

Secret negotiations between North Vietnam and the United States began in 1962. President Kennedy attempted to expand the Geneva Accords on Laos to guarantee the continued existence of South Vietnam, but Hanoi was not interested in any agreement short of reunification. Invitations to negotiate were interpreted as a sign of weakness by Hanoi, which refused to take seriously American threats to escalate the war. For the next dozen years, North Vietnam's position remained consistent; the American position fluctuated from year to year and from president to president.

President Kennedy's assassination "deprived the country of a debate among policy-makers over the wisdom of becoming more deeply involved" in Vietnam (p. 15). Lyndon Johnson "believed he had inherited a commitment, not the responsibility to decide if a commitment should be made" (p. 16). He did not encourage debate on Vietnam. Initially, he hoped for a negotiated settlement but rapidly became disillusioned as Hanoi leapfrogged between its public and private positions. In public, and with third parties from Harold Wilson to Herbert Marcovich, Hanoi indicated a willingness to enter talks if Johnson would unilaterally and unconditionally stop the bombing. Privately, Hanoi was inflexible and refused to negotiate. President Johnson believed if he stopped the bombing it would spell victory for the Communists. He was convinced that Hanoi was interested only in entering negotiations to facilitate a military victory. After the shock of the Tet offensive, Johnson, against his better judgment, was forced by domestic political circumstances to halt the bombing and negotiate.

When President Nixon entered the White House, he hoped diplomacy could end the war but believed "there could never be meaningful negotiations unless U.S. power remained credible." He was very aware of "Lyndon Johnson's experience: the gradual application of pressure simply didn't work on the North Vietnamese" (p. 107). When negotiations began, Nixon "operated on the premise that an agreement should be fundamentally in Saigon's interest" rather than Hanoi's (p. 109).

Secretary of State Henry Kissinger disagreed. Any agreement, according to him, had to be responsive primarily to Hanoi. He was more interested in improving relations with the Soviet Union and Communist China than in saving South Vietnam. Kissinger was determined to end the American involvement and in the summer and fall of 1972 used all his diplomatic astuteness to negotiate the Paris Agreement and persuade Washington and Saigon to accept it. The agreement was only a device for getting the United States out of Vietnam, because, "From the very day the Paris Agreement was signed, none of the parties to it expected its implementation" would end the war (pp. 157, 167). Kissinger defended it "largely in terms of what it did to restore the great-power equilibrium, not of what it achieved for the Vietnamese" (p. 85).

Goodman believes that negotiations were not only futile but actually prolonged the war. Hanoi interpreted them as a sign of weakness. "For Communists, negotiating is a tactic of warfare. The American conception of negotiations [on the other hand] is a process of bargaining and concession, and the outcome of negotiations is compromise not victory" (p. 5). Hanoi never intended to compromise its goal of reunification. Even though the Paris Agreement and subsequent congressional restrictions clearly weakened South Vietnam, Goodman believes Saigon's collapse in the spring of 1975 was inevitable. "U.S. goals—a successful outcome from a limited war and a political settlement—could be achieved neither by fighting nor by negotiating" (p. 24).

The text of *The Lost Peace* is only one hundred and eighty pages but Goodman provides a concise analysis of the negotiations from the American perspective. There are approximately a hundred pages of documents in eighteen appendixes. Although the documents are available elsewhere, the appendixes are a handy "reference text of the Paris Agreement and the key statements concerning its implementation" (p. 185).

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ARTHUR F. CORWIN, editor. *Immigrants—and Immigrants: Perspectives on Mexican Labor Migration to the United*

*States*. (Contributions in Economics and Economic History, number 17.) Westport, Conn.: Greenwood Press. 1978. Pp. xii, 378. \$18.95.

Well researched and often provocative, *Immigrants—and Immigrants* is Arthur F. Corwin's long-awaited book-length addition to the literature on Mexican migration to the United States. The study consists of six essays written solely by Corwin, three essays coauthored by Corwin, two biographical sketches of Mexican immigrants, and additional sections by Abraham Hoffman, Ernesto Galarza, and Paul Taylor. The book is largely the result of extensive archival and field research that Corwin pursued in the United States, in Mexico, and along the border for more than half a decade with support from the National Endowment for the Humanities.

In the initial sections of the book, Corwin presents a solid survey of the history of population movement across the Rio Grande. Particularly strong are chapters on the insoluble problem of accurately enumerating Mexican workers in the United States and the Mexican government's continual ambivalence toward the migration of its nationals. Of more limited value, however, is an essay by Corwin and Lawrence Cardoso on the causes of mass Mexican migration. In spite of its partial derivation from previously untapped Mexican archival material, this section sheds little new light on the "push" factors that have stimulated immigration over the decades. Hoffman's chapter on repatriation during the Great Depression also covers ground that has appeared in print elsewhere. More outstanding are Ernesto Galarza's comments on the Mexican migratory subculture. This section, a reprint of testimony Galarza presented before a Senate subcommittee in 1969, contains especially insightful discussions about the importance of family and crew leader in the lives of Mexican agricultural laborers.

The most controversial portions of this work are Corwin's critique of President Carter's Mexican immigration policy and essays on illegal immigration and the labor market in recent years, coauthored, respectively, with Johnny McCain and Walter Fogel. In treating the current immigration dilemma, Corwin abandons some of the historical perspective that he developed over the last several years. Apparently strongly influenced by numerous interviews with border patrol personnel, he blames much of this country's illegal alien problem on the Carter administration's support of a "human rights" approach to Mexican immigration that emphasizes amnesty rather than a "police solution." Specifically, Corwin attacks the "Affirmative Action bureaucracy," "hydra-headed poverty fighters," and

"scandalously lax" Great Society programs for enticing American and Mexican immigrant workers away from menial jobs and into self-perpetuating welfare programs. This loss of labor allegedly compels employers to seek constant fresh supplies of low-wage workers from below the border. While recent federal welfare policy undoubtedly has an impact on both the American labor market and immigration "pull" factors, the persistent economic pressures operating on prospective Mexican migrants long antedate Lyndon Johnson's Great Society. As Paul Taylor aptly remarks in the book's concluding essay, "plus ça change, plus c'est la même chose" (p. 347). Corwin, in his eagerness to take the Johnson and Carter administrations to task for not inventing a quick and easy restrictionist solution to a problem that has plagued American and Mexican policy makers for more than half a century, appears to forget that the United States has been and still is a safety valve for Mexico's poor.

Even more disturbing is Corwin's one-sided depiction of the role played in the contemporary immigration controversy by those he characterizes as "Chicano militants." "Ethnic militancy," he and McCain assert, "supported since the mid-1960s by the War on Poverty and the Civil Rights movement, is, in fact, inseparable from the present illegal alien phenomenon" (p. 72). According to Corwin and McCain, vehement pressure from the Chicano community in recent years has dissuaded Washington from enforcing immigration laws to such a degree that the Immigration Service has "lost control of the situation" in the barrios (p. 73). They imply unjustly that Chicano sensitivity, a sensitivity which must be attributed to past discrimination, exploitation, and excesses by *la migrá*, approaches paranoia.

Despite these weaknesses, *Immigrants—and Immigrants* contains a wealth of information on Mexican migration, both historical and contemporary, and merits inclusion in the collections of all serious students of American immigration history.

MARK REISLER

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## CANADA

CLAUDE GALARNEAU. *Les collèges classiques au Canada français (1620-1970)*. (Bibliothèque Canadienne-française.) Montreal: Fides. 1978. Pp. 287. \$7.95.

Readers should arm themselves with a magnifying glass and a copy of Jonathan Gathorne-Hardy's *Phenomenon of the English Public School* in order to appreciate this study. The glass will enable them to decipher the tables and figures; the book will pre-

vent them from the common error of finding French Canada unique (and therefore peculiar). The author has spent sixteen years unearthing everything knowable about the organization and development, the teachers, students, programs, and purpose of an institution transplanted from France in the mid-seventeenth century and only radically transformed in the 1960s.

Until the early twentieth century, the *collèges classiques* were the only centers of secondary education in French Canada. Until the 1960s the *collèges*—private, fee-paying institutions for the teaching of Latin, Greek, and *belles-lettres*—were the only route to a university. For three hundred years, every priest, lawyer, or doctor passed through the same program; the drop-outs went into business. Women, needless to say, were nowhere in sight: separate *collèges* for them were opened only in the twentieth century, after tremendous battles. That such a system should produce (besides this detailed and intriguing study) a clerical, elitist, conservative, even chauvinist society is not surprising.

What is surprising is the extraordinary vitality of the *collèges*. They reproduced themselves throughout the rural dioceses of Quebec; they followed French Canadian emigrants into Ontario and the West (but not into New England where the struggle for primary education absorbed all energies). They were agents of social mobility, turning farmers' sons into prestigious priests within a few years. Only in the twentieth century and only in some of the urban *collèges* of Montreal, suggests the author, did they become what one usually associates with private schools: institutions for perpetuating a secular elite. Urbanization, secularization, democratization spelled doom for the *collèges*.

Their endurance and eventual downfall can easily be traced to the power of the priests in French Canada. Certainly, the author has the sources to show feeble lay efforts at establishing secular schools, tiny numbers of lay teachers, continual infusions of priest-professors from France, and careful scrutiny by bishops to maintain and, after 1840, to enlarge their control. But these battles are chronicled rather than analyzed. One suspects that the author, like almost everyone of his generation, is too close to his subject. The clergy still bear the brunt of much social and national criticism in French Canada; this book reflects its time and place.

SUSAN MANN TROFIMENKOFF  
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HILDA NEATRY. *Queen's University. Volume 1, 1841-1917: To Strive, to Seek, to Find and Not to Yield*. Edited



by FREDERICK W. GIBSON and ROGER GRAHAM. Montreal: McGill-Queen's University Press. 1978. Pp. xvi, 346. \$25.00.

Hilda Neatby traced the growth of Queen's University, Kingston, Ontario, Canada from 1841 to 1917, and related the establishment and development of Queen's to the main currents of Canadian intellectual and social history. Obviously impressed by the rich traditions of Queen's University, Neatby prepared a scholarly work and made a valuable contribution to the history of higher education in Canada.

One of the striking characteristics in the development of Queen's during this period is the emergence of a strong tradition of academic freedom. There was also an emphasis on multiple curricula tailored to individual needs. After the establishment of the Alma Mater Society in 1859, Queen's was the first university in Canada to grant students the freedom to discipline themselves.

In the history of Canadian education, Queen's was a leader in university education for women. From 1884 to 1894, for example, Queen's maintained a Woman's Medical College. Queen's was also a leader in the development of graduate education in Canada. In 1889 doctoral programs in the humanities and sciences were established.

From 1877 to 1902, under the principalship of George Monro Grant, Queen's emerged as one of Canada's major universities. Considering the serious financial misfortunes and internal conflicts that Queen's confronted and overcame, this is indeed remarkable. The entire history of Queen's University in the nineteenth century is a vivid chapter in survival. The book contains long and detailed accounts of scholars, teachers, and lay supporters, whose pioneering efforts helped Queen's to survive.

Neatby explored the complex relationships with the Presbyterian Church and showed how the character of Queen's was influenced by its Scottish heritage. Although Principal Grant insisted that Queen's was not a denominational college, he still believed in a close link between theology and the arts.

Grant was convinced that Queen's should contribute to the development of Canada—a mission reflected in the national outreach of Queen's in drawing students from Western Canada and in the growth of extension studies. As part of this effort, Queen's established *Queen's Quarterly* in 1893, the first publication of its type in Canada. Grant, who became editor-in-chief, continued the publication for almost ten years without clerical assistance. *Queen's Quarterly* was supported by generous subscriptions from most of the staff of Queen's.

Grant was the last principal of the nineteenth

century. He strengthened the university and attracted the loyal and generous support of a growing number of benefactors and graduates. He repeatedly urged the graduates to give service and leadership to Canada, if they wished to prove themselves worthy members of Queen's University.

Hilda Neatby's clearly written history of the first seventy-six years of Queen's University was edited and completed, after her death in 1975, by Frederick W. Gibson and Roger Graham of Queen's history department. A second volume, covering the period from the end of World War I to the 1960s, is being prepared by Gibson.

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ROBERT CHOQUETTE. *Langue et religion: Histoire des conflits anglo-français en Ontario*. Ottawa: Éditions de l'Université d'Ottawa. 1977. Pp. 268.

Based on hitherto unused ecclesiastical documents, Robert Choquette's book (which also appears in an English translation) describes the bitter struggle between the Irish and French minorities of Ontario for the control of the Catholic institutions of that province in the first two decades of this century. A futile attempt to make the Catholic University of Ottawa, then bilingual, into a purely English institution produced the first open clash between the Irish and French clergy. A new test of strength over whether the new bishop of the largely French diocese of Ottawa should be Francophone or Anglophone was won by the Irish. Their most energetic and talented personality, Michael Fallon, on becoming bishop of the diocese of London, attacked the use of French in schools that Catholic children attended. In 1912 the Ontario government issued Regulation 17, which severely limited French as a language of instruction in its schools. Despite its being challenged in the courts, Regulation 17 prevailed throughout the war years. Eventually in the twenties the militant Franco-Ontarian opposition forced concessions; Rome appeased the French Canadian community by appointing a Francophone to the bishopric of Ottawa, and the Ontario government made teaching in French legal.

Choquette argues that the Irish, a quasi-nationality, with their own religion, language, and feeling of ethnicity, believed that their control over the Church, the vital instrument for the preservation of their identity, was threatened by the more numerous French. But the French too regarded the Church as crucial to their resistance to assimilation. Thus they wished for as much power within the Church as possible, and they also insisted that the French language be placed on an equal footing



with English in schools in French districts. The originality in Choquette's work is his demonstration that nationalist motives prevailed over religious ones within the Ontario Church and completely divided the hierarchy into Irish and French factions that assumed the leadership of their various communities.

Perhaps because his sources were Church documents, Choquette leaves the impression that the main clash between French and English took place within the Ontario Catholic Church. But this conflict was only part of a much wider one between French Quebec and English Canada about whether the French language was to be equal with that of English outside of Quebec. To many of the Quebec elite, such equity was a touchstone of whether Canada, from coast to coast, was as much their country as it was that of the English. Choquette does not demonstrate clearly enough that the strength of Bishop Fallon and his colleagues lay in their speaking for not only the Catholic Irish but also the great mass of Protestant Ontarians who rejected the principle of bilingualism. The majority of Ontarians believed passionately that, with the exception of Quebec, Canada was an English country; thus they bitterly opposed equal rights for French within the school system.

Still, the book has its principal merit as an absorbing study of the clash between two nationalities. Choquette, a Franco-Ontarian himself, is eminently fair; in fact, he is rather easy on the plain bigotry of the Irish clergy. Clearly, for both sides religious unity was not nearly as important as ethnic survival. Both Irish and French clergy addressed each other with venomous anger and truly nationalist passion. This happens when two weak but fairly matched minorities fight for the same piece of turf.

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GEORGE RADWANSKI. *Trudeau*. New York: Taplinger. 1978. Pp. xii, 372. \$14.95.

In 1978 Pierre Trudeau celebrated his tenth year as leader of Canada's Liberal party and as prime minister. A general election was expected in 1978 and it must have seemed a providential occasion to bring out a new, updated biography of the prime minister. But the election was postponed, political passions receded, and George Radwanski's *Trudeau* was left sitting on the beach.

Radwanski is an experienced journalist, familiar with Trudeau's native Montreal as well as with the complicated politics of the Province of Quebec. As his newspaper's Ottawa correspondent, he has had many opportunities to get to know his subject and

to study his actions from close at hand. When he decided to undertake a biography of the prime minister, his subject was cooperative; so were many of Trudeau's colleagues, friends, and relatives. From his circumstances and his interviews, Radwanski distilled a book, the first serious attempt to assess Trudeau in over five years.

Much of the story Radwanski tells is familiar: the precocious student, the outrageous adolescent, the world traveler, the opposition editor, and the principled antinationalist—these aspects of Trudeau's career have been journalistic staples for years. But when Trudeau became Liberal leader and prime minister in 1968 and embarked on a career as Canada's uncrowned philosopher-king, Radwanski leaves narrative behind for reflection. There follow chapters on Trudeau's political philosophy, on his administrative practices (widely reputed to be the most efficient in Canadian history), and on his personality ("What's he really like?"). The answers that Radwanski gives are both sharp and vague: sharp and clear where some knowledge already existed—yes, Trudeau is efficient, but yes, he is shy, and so forth—blurry where Trudeau chooses not to permit intrusions. The result is the amassing of much interesting detail on the organization of Trudeau's official life and on the interior of his cabinet. "I felt it very important to have a strong central government, build up the executive, build up the Prime Minister's Office, strengthen Parliament," Trudeau told the *New York Times* in 1968. And so, Radwanski makes plain, he did. But what he does not make plain is how he did it, who fell by the wayside, what the stages of growth really were in Trudeau's political life. Moments of high drama, such as the October crisis of 1970, when Trudeau proclaimed emergency measures against terrorism, become episodes in a larger, duller, drama. It is this narrative weakness that robs the book of much of its coherence and that, eventually, saps the reader's interest. It is all very good, very worthy, very dull, and, in the last analysis, meat for another book. For those who wish to write that book, Radwanski is essential. For those who wish to know about Trudeau's system of government, Radwanski provides a partial answer. But for those who want to read a biography of Trudeau, this is not the book.

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## LATIN AMERICA

ROGER BASTIDE. *The African Religions of Brazil: Toward a Sociology of the Interpenetration of Civilizations*. Trans-

lated by HELEN SEBBA. (Johns Hopkins Studies in Atlantic History and Culture.) Baltimore: Johns Hopkins University Press. 1978. Pp. xxviii, 494. Cloth \$28.50, paper \$8.95.

GEORGE EATON SIMPSON. *Black Religions in the New World*. New York: Columbia University Press. 1978. Pp. ix, 415. \$25.00.

*The African Religions of Brazil* is the English translation of the classic work by Roger Bastide originally published in 1960. Although the book is replete with rich ethnographic detail, its purpose is to demonstrate the usefulness of a sociological approach to the study of contact between civilizations. In the first part of the book, Bastide provides the finest short discussion of the African and Portuguese heritage that I know of. Throughout this discussion he lays the groundwork for the section to follow by pointing out the presence of multiple levels in every civilization and suggesting the problems in analysis that arise when such complexity is overlooked (for example, depth-sociology). In part two of the book he carries out his sociological analysis of Afro-Brazilian religions. He surveys their geographical diversity, their functioning, and how the African slaves' collective memory was maintained or changed in the New World. He further explores the influence of changing economic, social, and political structures on the religious sphere.

*Black Religions in the New World* is a new book that provides a view of the great variety of religions practiced by the descendants of sub-Saharan Africans dispersed throughout the Americas during the Atlantic slave trade. George Eaton Simpson divides the material geographically, discussing in greatest detail the Caribbean, the area to which he has made the most contributions. South America and North America receive two chapters each, one treating the historical churches (for example, Methodist, Baptist, Episcopalian) and the other chapter dealing with sects and cults (for example, *macumba*, *umbanda*, *xango*).

The point of view of Bastide markedly differs from that of Simpson. While the latter follows the Herskovitsian notion that Afro-Brazilian religions are syncretistic—a mixing of cultures—Bastide views them as an ongoing creative process in which actors interpret anew available cultural features in the context of socially available organizations. To Bastide the traditional values brought by civilizations to a contact situation can survive only if they are reincarnated in a set of institutionalized social relationships. It is through social structures that values function effectively and remain in being. Collective memory alone is an insufficient explanation for the permanence of African traditions in the New World. Whenever traits or symbols

could not be handed down because of gaps in the social context, values disappeared. If alternate routes could be created, values might be passed on in a new institutionalized context—as when the practice of divination passed from the *babalão* to the *babalôrixá*. When Africans were able to restructure their roles in accordance with the new social reality, the traditional values were preserved with fewer modifications—as in the move from lineage-oriented ancestor worship to ethnic-oriented worship of spirits providing levels of social participation approximate to the kin-based groupings made impossible by slavery. Even in this last case, Bastide argues convincingly that the creative efforts of individuals to adjust to new economic, social, and political situations are at least as important as their collective memory.

Bastide's book remains unsurpassed as a sociological analysis of Afro-Brazilian religions. The excellence of the translation does not make the book easy reading. The complexity of Bastide's analysis remains to challenge the reader to reflect on the evidence and to question facile interpretations that offer syncretism as if it were explanatory. Thoughtfully, Bastide shows the weaknesses of what he calls "plane sociology"—the use of single-level correlations between social variables. The usefulness of this approach is restricted to single cultural contexts. In analyses of contact between two or more cultures, "depth sociology" is more appropriate, permitting Bastide to demonstrate how *candomblé* became a valid and successful heir to the African village or town, how *macumba* represented the confusion of society in the face of rapid change, and how *umbanda* is a temporarily successful religion of a rapidly modernizing industrial power wherein major gaps in social enfranchisement still exist. The explanations blend in exquisite complexity the social, psychological, and ideological levels that impinge upon the process of creating symbols to mediate among these levels of the self and between the self and society at large.

*The African Religions of Brazil* provides an absolutely clear introduction to the sociology of religion and how the approach of depth sociology fits into the development of the field. Bastide's approach is explanatory rather than descriptive, sociological rather than chronological. The glossary is useful, as is the index of subjects.

In the first chapter of *Black Religions in the New World* Simpson asserts that the slavery experience is fundamental to an understanding of black religions in the New World. He provides a summary of the dispersion of Africans in the Americas—giving his estimate of nine to ten million Africans brought to the New World. About half of them went to South America (chiefly to Brazil), 42 percent to the Carib-

bean, and 7 percent to North America. In Catholic countries the slaves were given permission to form religious brotherhoods. Under the cover of worshipping the Christian saints, the slaves maintained many of their traditional practices. The high rates of slave importation into the Caribbean and Brazil also facilitated the maintenance of relatively pure African religious beliefs owing to a continual flow of native Africans into the plantations well into the late nineteenth century. In Protestant North America the evangelical orientation of these churches did not permit such continuity in religious practice. Sects and cults, strongly rooted in the Bible, eventually arose. They served the needs of the oppressed population, first under slavery and later in rural and urban settings.

Simpson continually reminds the reader that black religions are hybrid religions that constitute a response of marginalized blacks to their condition and within which they find a minimum of security, psychic release, and in-group social mobility. In fact, this assertion is concomitant to saying that culture is adaptive. The assertion has been made time after time by every sociologist, anthropologist, or psychologist who has studied Afro-American possession cults. Beyond this assertion, I found little sociological analysis in the book but much historical chronology arranged by religion and geographical area. At one point Simpson argues that participation in cults has both costs and benefits, but that given the disenfranchised status of most blacks the benefits far outweigh the costs. Unfortunately, nowhere in the volume does he provide the reader with a careful assessment of the costs and benefits that would demonstrate such an important assertion.

Given the broad coverage of religions, the author has had to use an enormous variety of sources of variable reliability. I found a general lack of critical evaluation of sources; the author does not question the possible biases of some accounts, the relative length of fieldwork on which the account is based, or the quality of the analysis. Simpson is familiar with a broad literature covering many countries, but I noted some important omissions in the references cited to classic works in the field of Afro-Brazilian religion (for example, Donald Pierson's *Negroes in Brazil*, Nina Rodrigues's *Os Africanos no Brasil* and *O Animismo Fetichista dos Negros Bahianos*, and Ruth Landes's *City of Women*). I cannot, however, speak of the coverage of the literature in other geographic areas.

Simpson tends to focus on exotic rituals and to neglect the complex social networks made possible within cults—such as the procurement of jobs and credit through cult networks. His discussions can easily be misinterpreted to suggest that cults do not

integrate members into the literate, urban-industrial world. As an alternative, Simpson suggests that the Protestant pentecostal churches, rather than the Afro-American cults, provide blacks with cultural values more appropriate to their social advancement. This flies in the face of the evidence that Afro-American cults such as *umbanda* have created schools, day-care centers, dental clinics, and hospitals to serve the urban proletariat. *Umbanda* has been shown to be capable of integrating African, European, and Indian cultural traditions within a modern, semiscientific religion capable of responding to new needs. Pentecostalism, on the other hand, has been noted to attract primarily persons of low socioeconomic status, to provide psychic release that differs little from possession cults, and to emphasize “being tested” and migrating in search of the promised land, which is conducive to geographic mobility but not to social mobility.

The bibliography is excellent for those wishing initial exposure to the religions of the African diaspora. Some of the notes could have been profitably incorporated into the text. The index of names is useful but the index of subjects seems organized around major subheadings used in the book—which made location of many discussions cumbersome or impossible. For example, “syncretism” and “collective memory,” both key concepts in the study of hybrid religions, are not indexed—but “acculturative process” by geographic region is, because it is a chapter subhead.

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CHARLES E. RONAN. *Francisco Javier Clavigero, S.J. (1731-1787), Figure of the Mexican Enlightenment: His Life and Works*. (Bibliotheca Institutii Historici S. I., number 40.) Chicago: Loyola University Press. 1977. Pp. xiv, 396. \$11.95.

The author gives us here a truly enlightening re-evaluation of the eighteenth-century Mexican Jesuit, Francisco Javier Clavigero. Although his labors in his native land were involved largely in work with urbanized Indians and with the teaching of philosophy, his fame rests primarily on two historical works that he produced during the enforced leisure of exile in Italy. Both appeared in Italian versions prepared by their author. The *Storia Antica del Messico* (4 volumes) was published in Cesena in 1780-81; the *Storia della California* was brought out posthumously by his brother in Venice in 1789.

In his first two chapters Charles E. Ronan traces Clavigero's life, first in Mexico and then in Italy after the expulsion of the Jesuit Order from the Spanish dominions. He was born in Veracruz on Sep-

tember 9, 1731, the son of a royal official who soon moved to Teziutlán in Puebla (1732) and then to Jicayán in Oaxaca (1734). Later his father sent him to Jesuit schools in Puebla. In 1748 Clavigero entered the Jesuit novitiate at Tepoztlán (near Mexico City) and went on from there to study philosophy in Puebla and theology in Mexico City, where he was ordained on October 13, 1754, at twenty-two years of age. After two further years of study he was assigned to work among the Indians of Mexico City, even though he had requested to be sent to the missions of Lower California. He was transferred to the school for Indians in Puebla briefly in 1762; then he was sent to Valladolid, or present-day Morelia (1763–66), and Guadalajara (1766–67). Here the great sweep of the expulsion caught him up on June 25, 1767, and he was shipped out—with stops in Veracruz, Havana, Cádiz, Puerto de Santa María (Spain), and Bastia (Corsica)—to Ferrara in Italy, where he arrived in mid-October 1768. In the summer of 1770 he moved to Bologna, which remained his principal place of residence until his death on April 2, 1787.

In Mexico he had belonged to the group of young intellectuals who endeavored to introduce the new philosophical and scientific ideas of the period into the prevailing scholasticism. In Italy he devoted his time largely to research and writing about his native land, a work to which he felt himself goaded by certain European writers of the time who maintained that all forms of life had degenerated in the Western Hemisphere. Answering the detractors was particularly his motive for writing his *Ancient History of Mexico*, a survey of the pre-Spanish history of Central Mexico. The work was enthusiastically received, and Clavigero prepared a Spanish version but, largely because of the opposition of the anti-Creole minister of the Indies, José de Gálvez, influenced by an exiled Spanish Jesuit, it was not published during Clavigero's lifetime and was later lost.

In a discussion of Clavigero's sources Ronan admits that Clavigero claimed greater intellectual originality than he actually offered. The work shows extensive and inadequately acknowledged dependence on the *Monarchia Indiana* of the early seventeenth-century Franciscan writer, Juan de Torquemada. Further, Clavigero claimed more extensive reliance on Indian source materials than can be verified. But he did recast the earlier works into a mold that was more acceptable to readers of eighteenth-century Europe. Ronan devotes a separate chapter to Clavigero's polemic with the detractors of the Americas, especially Buffon and Corneille de Pauw.

Clavigero's history of the Jesuit missions in Lower California, as Ronan shows in chapter five, is the more original of his two major works, as it incorporated information that he obtained from other

Jesuit exiles and from manuscript materials that remained unpublished until recent times.

In his philosophy of history, Clavigero clearly accepted the efficacious intervention of God and the devil in the course of human affairs, but he tried not to distort historical facts. In treating the Aztecs, however, he was influenced by a bias in their favor that sometimes led him to be less than critical.

In one instance Ronan may seem overly credulous, and that is in regard to the identification of Clavigero's supposed remains, which were transferred to Mexico City amid national honors in 1970. But even in this case he does not attempt to cover up the dubious methods of identification, and in a footnote he tucks away the statement that, "If the readers should entertain doubts about the authenticity of the remains . . . they should realize that it is really a point of little importance" (p. 110, n. 128). It is certainly a minor point in this work of great scholarly merit.

Ronan's study is extraordinarily thorough. He attempts to identify nearly everyone whose name appears in the text, and his extensive bio-bibliographical footnotes will be of great help to anyone who is dealing with Clavigero's sources and with the Jesuits and other important figures with whom Clavigero was in contact. It is a credit to Father Ronan, himself a Jesuit, that he has not attempted to hide Clavigero's human failings or the shortcomings of his writings and that he has exploded some of the myths that have grown up around Clavigero's name.

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JOHN W. F. DULLES. *Castello Branco: The Making of a Brazilian President*. Foreword by ROBERTO DE OLIVEIRA CAMPOS. College Station: Texas A & M University Press. 1978. Pp. xxi, 487. \$17.50.

This book's appearance in an important Brazilian national election year offers a certain irony. On the one hand, its interpretation will come as no surprise to readers already familiar with John W. F. Dulles's *Unrest in Brazil: Political-Military Crises, 1955–1964* (1970) or with the great lengths to which the Brazilian praetorian elite has gone over the last fifteen years to defend its role in the birth of their nation's most recent authoritarian dictatorship. General Humberto de Alencar Castello Branco (1897–1967), as the latecomer leader of a group of veteran military plotters who deposed constitutionally elected President João Goulart on March 31, 1964, launched an allegedly pre-emptive coup



against Brazil's populist chief executive. His decision to do so, according to both Dulles and those who seized power, came as a last resort and was in consonance with what that old-time conspirator and Castello's close friend, General Ademar de Queiroz, labeled a "movement to sustain democracy" (p. 310). Furthermore, Castello's motive was consistent with his lifelong devotion to the principle of legality, given that the Constitution of 1946 had again named the military as its trusted guardians. The author's supporting evidence for this timeworn "legalist" view of the 1964 *coup d'état* as well as Castello's personal, unswerving dedication to political neutrality as an army officer (with the intriguing exception of his signature on the generals' manifesto demanding constitutionally elected President Vargas's resignation in 1954) consists, significantly, of over one hundred interviews. Those conversations with almost all of the surviving military and civilian plotters likewise hold no surprises, although the reader will probably be stunned by the striking similitude of the *golpistas'* construction on the events preceding the Ides of March.

On the other hand, in extolling the moderation, even the "liberalism," of this most intellectual "little general"—a man who proudly returned from Paris with a bust of Napoleon in his baggage following graduation from the *Ecole Supérieure de Guerre* in 1938—Dulles raises a disturbing if only implicit further possibility. Could this extremely sympathetic and totally uncritical treatment of the only Brazilian general strong enough to unite a badly fragmented military against a popularly elected government not also be merely a foretaste of other rehabilitationist works yet to come? One would hope not. Yet the prefatory cue of Ambassador Roberto Campos (Castello's planning minister) that "history is likely to pass the verdict that the Revolution of 1964 was fortunate in having Castello Branco as its first leader" (p. xi) fills one with gnawing misgivings. The contemporary climate of hope surrounding the key issue of amnesty and the repeal of extensive legislation completely abrogating civil rights and liberties guaranteed by the Constitution of 1946 that today characterizes the "transition to democracy" in Brazil may nevertheless foster again a shortening of collective historical memory. Thus, it is not difficult to see how the tenth anniversary year of the "revolutionary" Brumaire of 1968, with its focus on the draconian legacy of Castello's 1967 successor, General Artur Costa e Silva, and the villainous military and civilian "hardliners," offers a convenient pause for reappraising Castello as the middle-of-the-roader authoritarian that he in fact was. That very dangerous tendency makes it very easy to forget the longer historical process that brought into being the bloody years initiated by the

closing of congress in 1968. For that reason, Castello Branco still deserves his due; in a real sense, he made it all very possible, and he should not be "let off" because he favored a more "constitutional" technocratic dictatorship.

Dulles abruptly closes his book short of even considering Castello's "presidency," following his "election" by the rump of state governors who survived the military purge after the coup. This legalist periodization avoids having to confront Castello's indispensable role in imposing unity during a crucial period when constitutional government was dismantled. Much more, however, is left unexplored. The most serious failure to analyze relates to the new evidence uncovered about "Operation Brother Sam" and the United States task force sent to assist the coup's plotters. Certainly, the official documentation that Phyllis Parker extracted from the Lyndon Johnson Library at the University of Texas for her 1964: *O papel dos Estados Unidos no golpe de estado de 31 de março* (1977) deserves as careful scrutiny as Castello's youthful love letters to his wife received at the book's outset. Instead, the author relies heavily on the manuscript version of Vernon Walter's *Silent Missions* (1978) and an interview with that former military attaché to the United States embassy during the coup period. If that rich vein of sources is left unexplored, it is, after all, as Ambassador Campos noted from his London embassy, because this book is intended as "an essay on the formation of a statesman, not a critique of governance" (p. xi).

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EDWARD DEW. *The Difficult Flowering of Surinam: Ethnicity and Politics in a Plural Society*. The Hague: Martinus Nijhoff. 1978. Pp. x, 234. f37.50.

Of all New World societies, the young nation of Suriname probably comes closest to fitting J. S. Furnivall's original concept of a plural society. Large population segments of African, East Indian, and Javanese descent are complemented by highly visible groups of Amerindians, Chinese, Europeans, and others. Each (in spite of significant internal cultural divisions) has managed to maintain considerable distinctiveness in everything from language and religion to notions of acceptable marriage or occupational choice. Shortly before Independence, the poet-politician Dobru optimistically characterized his motherland: "*wan bon/ someni wiwiri/ wan bon* (one tree/ so many leaves/ one tree)." The delicate problem of keeping these leaves vital while bringing the whole tree to flower is the subject of



Edward Dew's informative introduction to Suriname politics.

The first two chapters sketch in some necessary demographic, economic, and social background and offer an overview of the historical forces that shaped interethnic relations in the years preceding World War II. The next five chapters, which constitute the heart of the book, are arranged chronologically. In a direct, descriptive style, they recount what becomes a gripping story, building toward a climax in the events surrounding Independence in 1975. Chapter three describes the emergence of political parties based on ethnic considerations in the immediate postwar years, as suffrage rights that had previously been restricted to some 3 percent of the adult population suddenly became universal. Chapter four covers the period 1949-54, when intense jockeying for power occurred *within* the several ethnic/political blocs, as traditional cultural cleavages again came to the fore. (For example, Javanese "traditionalists" who faced west for prayer—since Mecca is west of Java—pitted themselves against "reformers" who now prayed toward the east; within the Creole group, distinctions of color, class, and religious sect re-emerged as highly charged political symbols.) Chapters five, six, and seven, covering the two decades preceding Independence, chronicle in detail the rise and fall of "consociationalism" as a political strategy as well as the final bitter struggle for the emergence of a new nation. A concluding chapter examines some of the implications of Suriname's recent experience in the light of more general theories of ethnic politics in plural societies.

Dew's data derive from some two years of research in Suriname and the Netherlands and are based on interviews with many of the principals in the story as well as on Dutch-language newspapers and documents. In tone, the book is unpretentious and nonpolemical; the author includes few anecdotes about personalities and maintains throughout a certain detachment from the events he describes. Yet, in almost every chapter, he offers revealing glimpses of the stuff of daily political intercourse (for example, in the heated debates about the colors of the proposed new flag or in the successful attempt by Amerindians to have the traditional Indian shield-bearers retained on Suriname's new coat of arms). Economic and social issues tend to receive only secondary attention, as Dew focuses his narrative on political actors and parties. But, if the book is a bit short on explanation and analysis, it successfully provides—in easily digestible form—a great deal of rich data. Students of plural societies and ethnic politics will be more than grateful to have this new case with which to conjure.

The deep, cross-cutting ethnic and cultural cleavages that characterize modern Suriname are but one of the heavy legacies of three and a quarter centuries of colonial rule. Dew's book is an important testimony both to the persistence of this heritage and to the courage of those Surinamers who have struggled, and are still struggling, to transcend it.

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RICHARD ALAN WHITE. *Paraguay's Autonomous Revolution, 1810-1840*. Albuquerque: University of New Mexico Press. 1978. Pp. x, 295. \$9.95.

After extensive research in several major depositories, Richard Alan White describes the revolution and Francia's dictatorship that resulted in political autonomy and a degree of economic independence for Paraguay. The book is divided into three parts: "The Colonial Heritage" interprets "classic colonial dependence" and the Jesuit missions; "The Revolutionary Process" traces the Creole revolt, the establishment of the various juntas, the emergence of Francia, and the consolidation and evolution of his power as absolute and perpetual dictator; and "The Struggle for Autonomy" explains Francia's economic and foreign policies. In the conclusion, White attempts to show how Francia selected, applied, and modified aspects of various contemporary political and economic philosophies. End materials include a chronology, nine statistical appendixes (some of which are also presented as graphs in the text), ample documentation, an extensive bibliography, and a short index. Adequate maps and expository notes complete the scholarly apparatus.

The author's preconceptions and bias are clear. He condemns the Spanish colonial system as one in which privileged elites exploited satellites in a "chainlike mechanism of appropriation" (p. 22) at the end of which were the Paraguayan people. Although White overworks such terms as "elites," "imperialism," and "colonial exploitation," he avoids the anachronistic trap of crediting Francia's regime with being an application of Marxist principles. As a revisionist—but by no means the first—he portrays Francia as a "popular" leader who led a "popular" revolution to establish a "popular" government. Actually, Francia was a well-educated, intelligent, and successful member of the Creole elite. He was a crafty manipulator, a skillful politician who destroyed the Spanish oligarchy and drastically reduced Creole wealth and power. Having established his supremacy, Francia ruthlessly crushed all opponents, especially those who plotted counter-revolution, that most terrible of all sins against a

"popular" revolt. Although White relates these facts accurately, he justifies them as a means by which El Dictador created a "popular" republic, but one in which the people never ruled.

Francia's policies probably improved the economic status of the people. Confiscated estates became state *estancias* or provided small farms for the landless. Government monopoly of trade produced enough profits and customs revenues to negate the need for general taxes. Francia's economic and foreign policies succeeded and his stern dictatorship prevented internal warfare. These are among the reasons why White defends the dictatorship, but he does not demonstrate any considerable improvement in social conditions; some progress might be inferred, however, from better economic conditions.

White has made good use of published works by Julio César Chaves and other writers. He also used the unpublished doctoral dissertation by John Hoyt Williams, which, together with several articles by Williams, covers practically all of the ground worked over by White. Nevertheless, this well-written book makes readily available statistical information laboriously compiled from masses of documents in the Archivo Nacional of Asunción. Although I disagree with White's interpretation of Francia as a popular leader, I recommend the book as a worthy contribution to Paraguayan history and as an aid to a more accurate understanding of El Supremo Dictador.

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JOHN W. FOX. *Quiche Conquest: Centralism and Regionalism in Highland Guatemalan State Development*. Albuquerque: University of New Mexico Press. 1978. Pp. xii, 322. \$12.95.

In dealing with ancient societies for which written records as well as material remains are available, archeologists generally have tended to adopt one of two approaches. These involve either (a) regarding such literary materials as constituting hypotheses about past conditions and events to be tested against the archeological data or (b) accepting them as historical facts to be used in interpreting archeological remains. John W. Fox's *Quiche Conquest* is an example of the latter, and possesses both strengths and weaknesses in consequence. Forming the core of the volume is a monograph-like catalogue of late pre-Hispanic archeological sites in the western highlands of Guatemala (pp. 16–269). The area is subdivided into five ethnic zones that are covered in separate chapters. Within each, a short introduction to the zone is followed by a listing of

its recorded sites, each of which is briefly treated in capsule-form sections headed topography and ecology; settlement description; artifacts; architecture and settlement pattern interpretation; ethnohistory. The coverage of ethnohistory, overall settlement (site) configuration, and specific architectural detail is good, but that of other material culture categories is severely deficient. The absence of any photographic illustrations, even of site settings or structural features, somewhat detracts from the overall value of the work. Each chapter concludes with a discussion in which the sites of that zone are considered individually and collectively with reference to its documents-based history. Together with the similar general discussion in chapter seven (pp. 275–303), these comprise the strongest aspect of *Quiche Conquest*. In them, Fox takes the lifeless and seemingly unrelated isolates represented by the numerous western highland sites and sets these within a historical framework derived from the exceptionally rich documentary sources covering that region. Intersite similarities and differences, intrasite formal characteristics, and site/settlement locations are discussed as functions of the twin processes of centralism and regionalism involved in the growth, expansion, and contraction of the Quiche state. The western highland sites thus are given a coherent historical significance that previously they have generally lacked.

I am considerably less enthusiastic about Fox's treatment of Quiche origins. This involves an uncritical acceptance of the documentary source-based hypothesis of Robert M. Carmack (*Toltec Influence on the Postclassic Culture History of Highland Guatemala* [1968]) proposing the lowlands of the southern Gulf Coast as the original homeland of this people. This quite plausible hypothesis well deserves archeological testing. Fox's approach, however, is to assume its *a priori* validity and employ it for explanatory purposes. The results are less than satisfactory. The few architectural coincidences cited are vague and superficial, their weight unconvincing. There is a significant difference, for example, between settlements positioned on ridges or hilltops for defensive reasons and those so situated because the intervening lowlands are inundated throughout much of the year. The virtually total absence of correspondences—or at least discussion thereof—between the Quiche and their putative lowland antecedents in any other material cultural categories further weakens the credibility of the architectural assessments.

As a basic index of Postclassic settlement/site locational data for the western highlands of Guatemala, *Quiche Conquest* is invaluable. With respect to historical content, it must be regarded with mixed feelings. Its provision of a coherent, historical-pro-

essual framework within which to approach the archaeological remains of the region is a welcome and important contribution. Its treatment of Quiche origins, however, must be viewed with healthy skepticism at best.

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M. A. LOWELL GUDMUNDSON KRISTJANSON. *Estratificación socio-racial y económica de Costa Rica, 1700-1850*. (Estudios Sociopolíticos, number 3.) San José, Costa Rica: Editorial Universidad Estatal a Distancia. 1978. Pp. 178.

The author's intention is to reinterpret some ethno-social concepts, such as the preponderance of racial democracy, apparent in Costa Rican historiography. Although the work makes some contribution, it promises more than it delivers.

In the first study, Kristjanson explains how from 1648 to 1824 manumission and miscegenation became means of social mobility. In a society whose economy was stagnant, slaves were not extensively exploited and their manumission, though scant, was constant. Accordingly, freedom took on added significance and the former slave was capable of changing his status. The only drawback was the static economy that impeded his rapid integration into a higher socioeconomic level because wage laborers could not be readily absorbed. Likewise, when miscegenation occurred, the darkest of the new social group tended to remain either unmarried and childless or in the lower class procreating illegitimate children. Costa Rican society thus tried to whiten its members through biological selection. Although the author may not be adding novel concepts to the dynamics of Latin American racial mixture, his use of quantitative analysis shows how, within a restricted socioeconomic setting, miscegenation lessened the degrading aspects of slavery while accelerating social integration.

Kristjanson's study has some weak points: the number and occupation of the slaves are not given; the position of the Amerind is ignored; and, since the structure of the economy and the purpose of slave labor are not provided, data about other aspects of slave life become mute witnesses to an unknown situation (p. 25).

The second study sheds light on the creation of economic hegemony through the accumulation of land and cattle and the increase of absentee ownership. It deals specifically with the establishment of an *hacienda* as a charitable enterprise whose income was used to help maintain a hospital. The study is marred, however, by the misunderstanding of property rights and contractual obligations. For instance, the sale of the *hacienda* by its first and only administrator was not an attempt to further the accumulation of property in the hands of the few but to insure that the hospital would continue to receive its yearly income (pp. 82, 90, 105), as provided under the terms of the *hacienda's* establishment.

The third study determines how socioeconomic stratification took place just before coffee production influenced wealth distribution. Based on a detailed 1838 census report of a town, the study is a good analysis, showing conclusively that colonial society was not a rural democracy, as traditionally believed. Although there are several questions left unanswered (such as the merchants' role, pp. 147-48), the work should provoke historians to continue demographic studies. This section also has major weaknesses: confusion about property rights (p. 144) and poorly presented tables (for example, pp. 41-43, 138, 140, 145). Additionally, the work should have been carefully edited to correct grammatical errors.

In summary, Kristjanson makes great strides in this work but much more remains to be done.

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## Collected Essays

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These volumes, recently received in the AHR office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other similar volumes that are amenable to reviewing will be found in the review section.

SHIRLEY SUGERMAN, editor. *Continuities and Discontinuities: Essays in Psychohistory*. With an introduction by DAVID BAKAN. Madison, N.J.: Drew University Publications. 1978. Pp. xvi, 124. \$5.75.

PHILIP K. JENSEN, *Psychohistory?* DAVID BAKAN, *The Experimental and Historical Methods in Psychology*. ROGER W. WESCOTT, *The Personality of Cultures*. NELSON W. THAYER, *Ideology, Religion, and Identity*. WOLFGANG M. ZUCKER, *The Return of the Demons*. SHIRLEY SUGERMAN, *The Metamorphoses of Madness: A Study in Psychohistory*. DAVID GRAYBEAL, *What Happens in the Village*. JAMES M. O'KANE, *Ethnic Assimilation in American Social History: The Journey from the Bottom*. JOHN OLLOM, *Thomas Kuhn as Symptom and Portent*. ROBERT CHAPMAN, *The Psychology of Semantic Change*. OTTO KLINEBERG, *Psychology and the Future*.

JUAN J. LINZ and ALFRED STEPAN, editors. *The Breakdown of Democratic Regimes*. Four volumes in one. Baltimore: Johns Hopkins University Press. 1978. Pp. x, 124; 218; 208; v, 168. Cloth \$35.00; paper volume 1, \$2.95; paper volumes 2-4, \$3.95 each.

JUAN J. LINZ, *Crisis, Breakdown, and Reequilibration*. PAOLO FARNETI, *Social Conflict, Parliamentary Fragmentation, Institutional Shift, and the Rise of Fascism: Italy*. M. RAINER LEPSIUS, *From Fragmented Party Democracy to Government by Emergency Decree and National Socialist Takeover: Germany*. WALTER B. SIMON, *Democracy in the Shadow of Imposed Sovereignty: The First Republic of Austria*. RISTO ALAPURO and ERIK ALLARDT, *The Lapua Movement: The Threat of Rightist Takeover in Finland, 1930-32*. JUAN J. LINZ, *From Great Hopes to Civil War: The Breakdown of Democracy in Spain*. PETER H. SMITH, *The Breakdown of Democracy in Argentina, 1916-30*. ALEXANDER W. WILDE, *Conversations among Gentlemen: Oligarchical Democracy in Colombia*. DANIEL H. LEVINE, *Venezuela since 1958: The Consolidation of Democratic Politics*. ALFRED STEPAN, *Political Leadership and Regime Breakdown: Brazil*. GUILLERMO O'DONNELL, *Permanent Crisis and the Failure to Create a Democratic Regime: Argentina, 1955-56*. JULIO COTLER, *A Structural-Historical Approach to the Breakdown of Democratic Institutions: Peru*. ARTURO VALENZUELA, *Chile*.

JULIAN I. SIMON, editor. *Research in Population Economics: An Annual Compilation of Research*. Volume 1,

1978. Greenwich, Conn.: JAI Press. 1978. Pp. ix, 347. \$28.50.

IRMA ADELMAN and SHERMAN ROBINSON, *Migration, Demographic Change and Income Distribution in a Model of a Developing Country*. JOHN J. SCULLY, *The Influence of Family Size on Efficiency within the Farm—an Irish Study*. STEPHEN P. DRESCH, *Ability, Fertility and Educational Adaptation*. ADAM M. PILARSKI, *The Impact of Fertility on Hours of Work: A Cross-Country Comparison*. MARCUS FELSON and KENNETH C. LAND, *Social, Demographic and Economic Interrelationships with Educational Trends in the United States, 1947-1974*. DOUGLAS LOVE and LINCOLN PASHUTE, *The Effects of Population Size, Growth, and Concentration upon Scientific Productivity*. COLIN CLARK, *Population Growth and Productivity*. K. JONES and A. D. SMITH, *The Economic Impact of Commonwealth Immigration*. JULIAN I. SIMON, *An Integration of the Invention-Pull and Population-Push Theories of Economic-Demographic History*. LINCOLN PASHUTE, *Economics versus Racial Discrimination in the Provision of Birth-Control Services in the United States*. JOSEPH J. SPENGLER, *Population Phenomena and Population Theory*. MARIO I. BLEJER, HARRY G. JOHNSON, and ARTURO C. PORZECANSKI, *An Analysis of the Economic Determinants of Legal and Illegal Mexican Migration to the United States*. MARTIN O'CONNELL, *The Effect of Changing Age Distributions on Fertility: An International Comparison*. EILEEN CRIMMINS-GARDNER and PHYLLIS A. EWER, *Relative Status and Fertility*. ARLAND THORNTON, *The Relationship between Fertility and Income, Relative Income, and Subjective Well-Being*. MAURICE M. MACDONALD and RONALD R. RINDFUSS, *Relative Economic Status and Fertility: Evidence from a Cross-Section*. DANIEL A. SEIVER, *Which Couples at Given Parities have Additional Births?* FRANK D. BEAN, SUSAN H. COCHRANE, HOWARD SAVAGE, and CHARLES H. WOOD, *Income and the Supply and Demand for Children: An Analysis of Wanted versus Unwanted Fertility*. MARK R. ROSENZWEIG, *The Value of Children's Time, Family Size and Non-Household Child Activities in a Developing Country: Evidence from Household Data*.

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the Pseudo-Dionysius by University Teachers in the Later Middle Ages. FRANTIŠEK ŠMAHEL, Prolegomena zum Prager Universalienstreit: Zwischenbilanz einer Quellenanalyse. MIECZYSLAW MARKOWSKI, Astronomie an der Krakauer Universität im XV. Jahrhundert. JEANNE VIELLIARD, Le registre de prêt de la bibliothèque du collège de Sorbonne au XVe siècle. NEIL R. KER, Oxford College Libraries before 1500. LAETITIA BOEHM, Humanistische Bildungsbewegung und mittelalterliche Universitätsverfassung: Aspekte zur frühneuzeitlichen Reformgeschichte der deutschen Universitäten. AGOSTINO SOTTILI, Le natio germanica dell'Università di Pavia nella storia dell'umanesimo. GIUSEPPE BILLANOVICH, L'insegnamento della grammatica e della retorica nelle università italiane tra Petrarca e Guarino. FRANK BARON, The Historical Doctor Faustus at the University of Heidelberg. JACQUES PAQUET, L'universitaire 'pauvre' au Moyen Âge: problèmes, documentation, questions de méthode. GUY FITCH LITTLE, The Social Origins of Oxford Students in the Late Middle Ages: New College, c. 1380-c. 1510. HILDE DE RIDDER-SYMOENS, Les origines géographiques et sociales des étudiants de la nation germanique de l'ancienne université d'Orléans (1444-1546): aperçu général. ALEKSANDER GIEYSZTOR, Origines sociales et nationales du corps universitaire de Cracovie aux XIVe et XVe siècles. EDWARD DE MAESSCHALCK, Scholarship Grants and Colleges Established at the University of Louvain up to 1530. VERN L. BULLOUGH, Achievement, Professionalization, and the University. ANTONY BLACK, The Universities and the Council of Basle: Collegium and Concilium. ANDRÉ GOURON, Le recrutement des juristes dans les universités méridionales à la fin du XIVe siècle: pays de canonistes et pays de civilistes? ROBERT FAVREAU, L'Université de Poitiers et la société poitevine à la fin du moyen âge. RAFAEL DE KEYSER, Chanoines séculiers et universités: le cas de Saint-Donatien de Bruges (1350-1450). JACQUES PYCKE, Les chanoines de Tournai aux études, 1330-1340. HANS-JÜRGEN BRANDT, Universität, Gesellschaft, Politik und Pfründen am Beispiel Konrad von Soltau (1407).

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MADELEINE PELNER COSMAN, Machaut's Medical Musical World. BERNARD R. GOLSTEIN, The Role of Science in the Jewish Community in Fourteenth-Century France. JOHN E. MURDOCH, *Subtilitates Anglicanae* in Fourteenth-Century Paris: John of Mirecourt and Peter Ceffons. JOHN D. NORTH, Kinematics—More Ethereal Than Elementary. EDWARD GRANT, Scientific Thought in Fourteenth-Century Paris: Jean Buridan and Nicole Oresme. BERT S. HALL, Giovanni de'Dondi and Guido de Vigevano: Notes Toward a Typology of Medieval Technological Writings. MARCEL THOMAS, French Illumination in the Time of Guillaume de Machaut. SUSAN O. THOMPSON, Paper Manufacturing and Early Books. WILLIAM CALIN, The Poet at the Fountain: Machaut as Narrative Poet. SARAH JANE MANLEY WILLIAMS, Machaut's Self-Awareness as Author

and Producer. DANIEL POIRION, The Imaginary Universe of Guillaume de Machaut. KARL D. UTTI, From *Clerc* to *Poète*: The Relevance of the *Romance of the Rose* to Machaut's World. KEVIN BROWNLEE, The Poetic Oeuvre of Guillaume de Machaut: The Identity of Discourse and the Discourse of Identity. MICHELLE A. FREEMAN, Froissart's *Le Joli Buisson de Jonece*: A Farewell to Poetry? RICHARD R. GRIFFITH, Bertilak's Lady: The French Background of *Sir Gawain and the Green Knight*. ROBERT MARK, Innovation in High Fourteenth-Century Gothic: The Church of St. Ouen, Rouen, and the Cathedral of Palma, Majorca. JONATHAN B. RIESS, French Influences on the Early Development of Civic Art in Italy. LEO MUCHA MLADEN, Bartolus the Man.

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### TO THE EDITOR:

We find that Seymour Byman's article, "Ritualistic Acts and Compulsive Behavior: The Pattern of Tudor Martyrdom" (*AHR*, 83 [1978]: 625-43), raises a number of historical and psychological problems. Unfortunately, as important as the subject is, it is obscured by doubtful psychohistorical methods that occasion misleading conclusions. We hope that these conclusions are not rooted in fundamental misunderstandings of historical and psychological inquiry.

Professor Byman's article turns upon the statement that "many [Protestant martyrs] were plagued by the suspicion that the actual motive in accepting martyrdom was suicide" (p. 625). He then explained that the candidate-martyrs engaged in compulsive, ritualistic behavior in a neurotic attempt to ward off fear and anxiety about suicidal impulses. "In contrast to normal adaptive ritualistic behavior, the pattern devised by the martyrs was pathological, both in its source and in its pattern" (p. 626). On several important points we disagree.

In the first place, although Professor Byman as-

serted that the martyrs were plagued by suicidal impulses, he presented no evidence that any of the martyrs were, in fact, so troubled. There is no reason for us to accept *a priori* assumptions of psychological motivation without evidence from the subjects—evidence in the form of diaries, letters, reported conversations, or the like. A basic principle of psychoanalytic interpretation is that the analyst cannot assume the presence or the significance of a motivational drive unless and until the subject presents multiple evidence of that motivation, over time and in relation to multiple behaviors in different contexts. Thus, a single statement or behavior is not sufficient to determine motivation because the determination of motivation is a psychological inference built from multiple pieces of data that lead to a justifiable conclusion. Without such evidence, any scholar risks an exercise in psychological speculation rather than in psychohistorical explanation.

Our second problem derives from the assertion that the martyrs' (alleged) fear of suicide was, itself, an intrapsychic drive toward self-destruction. Yet much of the data that Professor Byman offered refers to social perceptions of martyrdom rather than to intrapsychic dynamics. We might as easily argue that the question of suicide was not an intrapsychic conflict in the martyr but, instead, the martyr's fear that his act might be socially misinterpreted. Our argument is based on Byman's own statement that it was possible for the public to interpret martyrdom as a suicidal act. We are thus led to conclude that the compulsive rituals that Byman catalogued might as easily be explained as an effort to establish and reinforce a proper social interpretation of martyrdom.

Third, we are troubled by the article's conclusion, without a compelling psychological justification, that suicidal motivation was the primary motivation for both ritualistic acts and compulsive behavior. A fundamental psychodynamic concept, known as "multiple determination," holds that no given set of behaviors is due to a single psychological drive or motivation. Behavior is, instead, the common pathway that results from the confluence of multiple



drives and motivations. Just as no one drive or motivation is sufficient to produce a set behavior, so, too, different motivations or drives may produce the same set behavior at different times. Thus we must ask which complex of drives or motivations produces a particular behavior at a particular moment. This analysis requires dissection of the relative significance of each drive or motivation contributing to a given behavior. For, although detectable, particular drives or motivations may be relatively insignificant variables in the genesis of a set behavior. Therefore, even if Professor Byman were to establish a suicidal motivation among the martyrs, we would still be faced with the question of assessing its relative significance. It is difficult to accept that suicide was the sole or primary determinant of martyr behavior.

The allegation that compulsive rituals of the martyrs were pathological and neurotic rather than normal adaptive behavior is not supported with adequate evidence. Freud himself fell into the trap of isolating and explicating the psychological roots of behavior in terms of neurosis alone. Perhaps a more apt title for *The Psychopathology of Everyday Life* might have been "The Psychodynamics of Everyday Life" because both instinctual and conflictual elements of the intrapsychic process are manifest. Professor Byman has accurately described the way in which obsessive ritual behavior neutralizes aggressive instincts. He has not, on the other hand, made a case for calling such behavior neurotic. The psychodynamics (not the psychopathology) of obsessive behavior were not unique to the martyrs. Indeed, the psychoanalyst George Vaillant has shown that all normal persons use all of the same ego defenses employed by neurotic persons (*American Journal of Psychoanalysis*, 19 [1971]: 110-15). The neurotic tends to fix on one set of ego defenses without regard to the situation, while the normal person will utilize many such defenses. Without evidence of the frequency or the distribution of ego defenses used by the martyrs—evidence that is, perhaps, unattainable—the psychohistorian cannot justifiably conclude that the obsessional behavior of the martyrs was neurotic rather than normally adaptive.

An important subject such as the study of martyr behavior would benefit from the application of the concepts of social psychology. Much of the ritual of martyrdom was occasioned by socially determined role behavior. An examination of the evidence of the martyrs' "culturally constituted" defenses would introduce social role analysis as a counterbalance in an exclusively intrapsychic analysis of martyr behavior. We entirely agree with Professor Byman's caveat that "it should not be forgotten that the sixteenth-century milieu of these martyrs was quite different from our own" (p. 642). The introduction of social psychology to this study would have given greater substance to that statement.

Our contemporary aversion to the realities of death and dying can obscure sixteenth-century attitudes about death. In *Western Attitudes Toward Death from the Middle Ages to the Present* (1974), Philippe Ariès aptly captured the mentality of the sixteenth century. The moment of death was the final test of motivation of man seeking God. "His attitude during this fleeting moment will erase at once all the sins of his life if he wards off temptation or, on the contrary, will cancel out all of the good deeds if he gives way. The final test has replaced the Last Judgment" (p. 37). From this point of view, the sixteenth-century martyrs did not display pathological behavior but acted congruently with a culturally contemporary attitude toward death.

We believe that psychohistorical studies of martyrs or of martyrdom are of tremendous importance, but we also believe that a satisfactory approach to martyr behavior requires a broader range of methodologies than is employed in this article. Our understanding is not significantly enlarged by simply labeling the martyr a neurotic.

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#### PROFESSOR BYMAN REPLIES:

Attacks on psychohistory appear in various guises, the most insidious of which are the demands placed on psychohistorical investigations that are not placed on other historical inquiries. This is the approach used by Lamar M. Hill and E. Mansell Pattison in their criticism of my article. To demand the "dissection of the relative significance of each drive or motivation contributing to a given behavior" is to demand the impossible. One can only assume that Hill and Pattison's purpose is to discredit psychohistory. Despite their claim that psychohistorical studies are important, their insistence on diluting psychohistory with a variety of other methodologies is a movement toward barren historical methods that would denude scholarship of its psychological and historical impact.

Professors Hill and Pattison begin their attack by citing lack of evidence to prove a suicidal pattern. Yet the documentation of the martyrs' suicidal behavior is ample; the article is replete with references that document self-destructive motivation. I can only conclude, therefore, that Hill and Pattison arrived at their erroneous assumption that suicidal impulses were lacking in the martyrs by failing to read—I hope not deliberately misreading—my references. The purpose of my article was not to prove suicidal behavior but to demonstrate the ramifications of repressed suicidal longings for ritualization and compulsive behavior. The self-destructive pattern of these martyrs was indicated in numerous

other articles that are cited in the references (see my "Suicide and Alienation: Martyrdom in Tudor England," *Psychoanalytic Review*, 61 [1974]: 355-73; "Guilt and Martyrdom: The Case of John Bradford," *Harvard Theological Review*, 68 [1975]: 305-31; and "Child Raising and Melancholia in Tudor England," *Journal of Psychohistory*, 6 [1978]: 67-92). Had Hill and Pattison taken the time to read the references, they would have found the "diaries, letters, reported conversations, or the like" so necessary to scholarship of this type. The evidence contained in the Harleian manuscripts at the British Library, the volumes of Foxe, and the publications of the Parker Society overwhelmingly indicate a pattern of suicide on the part of the martyrs. The Marian martyr, John Bradford, is a case in point. Racked by guilt, Bradford attempted to become a slave. Not able to find a willing earthly master, he badgered the Marian authorities until they imprisoned and eventually killed him. The pain of death was considerably allayed by Bradford's belief that martyrdom was the "most safe kind of dying." The list of martyrs who chose to die is numerous: Anne Askew, Julius Palmer, Robert Glover, and Richard Gibson are but a few of many.

Given that these martyrs were suicidal, I attempted to illustrate how the sixteenth-century milieu affected their behavior. Because the punishments for the suicide meant a loss of inheritance for their heirs and a horrible punishment in hell for themselves, the martyrs had to repress any overt thought of suicide. They wanted to die, but they could not directly kill themselves. But others, agents of the Anti-Christ, could kill them. The martyrs' unconscious ambivalence toward their death wishes led to extreme anxiety that was, in turn, manifested by highly ritualized, compulsive behavior. They slavishly copied the words and actions of the early Christian martyrs who were not under the ban of suicide, a ban introduced at the time of Augustine.

So much for the evidence. Professors Hill and Pattison also take issue with my methodology, a more serious question. I am deeply troubled by the implication of their statements, both for the field of psychohistory and for the study of history in general. Their attempt to water down the psychohistorical methodology with the inclusion of a smorgasbord of techniques would result in an eclectic study with few conclusions. Let me hasten to add that I do not deny the importance of a multidisciplinary approach; the psychoanalytic technique does not, in itself, preclude additional interpretations. Even though in my studies, psychoanalytic theory is the primary tool to the interpretations of the times, I have not excluded consideration of the rapidly changing political and religious events, the economic status and class of the martyrs, the role of women in society, family patterns in the sixteenth century, and so forth. The

psychoanalytic technique is simply another means of comprehending the incredibly complex forces that shaped the personality of the martyrs. I have chosen to make it the method that encompasses but does not exclude the others.

In their attack on methodologies that suggest "an intrapsychic drive," Professors Hill and Pattison ask for a total quantificational understanding of each drive and motivation that comprise the human personality. Are we to chart these drives on a computer, deciding like an EKG whether or not the personality is healthy or neurotic? Their call for "evidence of the frequency or the distribution of ego defenses" invalidates all previous works of psychohistory. According to these standards, the traditional studies of Eric Erikson, Bruce Mazlish, and Peter Loewenberg are no longer valid.

The martyrs' actions were obviously multidetermined. Nevertheless, one or two decisive forces shaped their behavior. Since neither analysts nor historians can discover all of the threads that comprise human behavior, we must concentrate on one or two. And the task of the psychohistorian is even more difficult than that of the analyst, for the total picture of human behavior can rarely be reconstructed. Should, then, *Young Man Luther* be discarded because Erikson concentrated on Luther's relationship with his father and neglected the tie of Luther and his mother? Of course not. A historian can do no more than interpret the extant information.

In Tudor England, despite the importance of dying well, despite the religiously oriented society with its message of salvation, only a few were martyred. Most chose to struggle with life rather than to capitulate to death. How the martyrs reflected their society and how they differed from other men and women in that society is the warp and woof of history.

SEYMOUR BYMAN  
Winona State University

#### TO THE EDITOR:

In "Magic, Astrology, and the Early American Religious Heritage" (*AHR*, 84 [1979]: 317-46), Jon Butler displayed a misunderstanding of the nature of religion and so did not demonstrate the difference between religion and magic. Religion, as the etymology implies, deals with the binding to or union with the divine, under whatever name one may wish to call it. In Butler's period, the divine was largely, but not exclusively, expressed in the Judeo-Christian tradition. Here the basic tenets are "Thou shalt have no other gods before me" and "Thou shalt love the Lord thy God. . . ."

Cutting through Professor Butler's euphemisms, one finds that he defined religion as the manipulation of the divine for human comfort and conve-

nience. It is not possible to square this definition with the love of the divine. All of the aspects of love—devotion, forgiveness, inspiration, generosity, joy, fulfillment, strength, and the like—are denied if the lover is motivated by selfish gain. Few would be so foolish as to treat a beloved human as a satisfaction mechanism, but Butler seems to have thought that God can be conned. Butler can, indeed, find plenty of company in this opinion, even among the adherents of organized religion, but this fact only serves to illustrate the variety of human folly.

To be more exact, in presenting his material on magic, divination, alchemy, herbal medicine, and so forth, Professor Butler dealt with the history of technology, the function of which is to deal with phenomena for the benefit of mankind. Butler is not even dealing with the history of science, for technology is merely a self-serving branch of science. Science in the hands of a Newton, an Einstein, or a Teilhard de Chardin is, like religion, a way of approaching the divine.

Professor Butler was quite right, of course, not to equate the history of religion with church history, but I do not know who does. In colonial history, the student must seek the divine not only in the fellowship of the churches and the Society of Friends but also, for example, in the elegance of a Shaker chair, the harmonious life style of a colonial farmer, the music of the Moravian trombonists, and the balanced rationalism of the political writers.

It is worthwhile being clear about the human manifestations of divinity, for many and especially the young look to historians for the definition of values. As today the fascination with technology wanes, the greed for unlimited economic expansion palls, and the gyrations of political leaders become increasingly fantastic, young people seem to be turning more and more to the inner life of contemplation and religion. Learning to live in the noosphere is the need of the future, and history has much to teach in that area.

GEORGE T. PECK  
*Greenwich, Connecticut*

TO THE EDITOR:

In "What Fascism Is Not: Thoughts on the Deflation of a Concept" (*AHR*, 84 [1979]: 367–88, 394–98), the *AHR Forum* this past April, Gilbert Allardyce questioned the existence of "unifascism" on the grounds of an alleged lack of a "certified cluster of shared traits" (p. 378) among the fascist movements. Surely, such mental anguish can be avoided if we recognize that these vexatious "isms" are often best explained not by what they propose but by what they oppose. Socialism began and remains quintessentially anticapitalist no matter how many maximalist and minimalist programs proliferate.

Similarly, fascism everywhere in Europe between the wars constituted a reaction against the dominant materialist culture; this is why *all* fascists were sworn and consistent foes of both bourgeois liberalism and dialectical materialism. How this antimaterialism was expressed—by a flight from modernity or into new forms of modernism, for example—varied according to national circumstances, but did not change the basic thrust and identity of facism at large. How odd that an article bearing the title "What Fascism Is Not" should skirt the obvious and definitive issue of what fascism was against.

ALAN CASSELS  
*McMaster University*

TO THE EDITOR:

I could only now read the review of my book, *Hsin-Lun (New Treatise) and Other Writings by Huan T'an (43 B.C.–28 A.D.)*, annotated translation with index by Timoteus Pokora (1975), written by Chi-yun Chen (*AHR*, 81 [1976]: 1196). I find in the review a fair account of the life and ideas of Huan T'an without, however, any statement about whether this account is based upon my book or belongs to the reviewer. As a matter of fact, there is only one sentence appraising my work: "According to Pokora, Huan T'an seemed to hint at the modern Maoist 'mass line' (p. xxi), though this interpretation seems a bit far-fetched and out of context." I have to state with emphasis that the above-quoted statement cannot be found anywhere in my book and the responsibility for it belongs exclusively to the reviewer.

TIMOTEUS POKORA  
*Prague, Czechoslovakia*

PROFESSOR CHEN REPLIES:

I apologize to Timoteus Pokora for my use of the expression "Maoist 'mass line.'" What I should have done, space allowed, was to quote Dr. Pokora's exact words: "Some of Huan T'an's political criticisms sound remarkably modern. 'Also to let the enlightened and the wise to plan affairs and then bring them down to the masses will certainly be inadequate.' . . . gave priority to the people . . . in harmony with the mind of the people" (pp. xxi–xxii). The correct translation of the single-quoted text (from Yen, 13. 5a), according to the intent and context, should be something like, "Furthermore, to commission the enlightened and the wise to devise a plan but then require them to level it off with the multitude (for example, subject it to the criticism by the mediocre majority of their official colleagues) is something that surely will not do." (For the context, see *Writings by Huan T'an*, p. 17.) The meaning of this passage is the opposite of what Pokora made it on page xxi.

The intent of my review was not the correction of Dr. Pokora's translation (corrections have been made in other reviews), but to summarize the significance of Huan T'an's work as introduced and translated by Pokora.

CHI-YUN CHEN  
University of California,  
Santa Barbara

TO THE EDITOR:

The review of my *History of Middle Europe from the Earliest Times to the Age of the World Wars* by Lawrence P. Meriag of the University of Kansas (*AHR*, 82 [1977]: 1013-14) is based exclusively on false allegations and misstatements of fact.

(1) The Treaty of Schönbrunn, 1809, was not "re-named" the Treaty of Vienna by me but by generations of historians, among them the late Louis R. Gottschalk, president of the AHA in 1953 (see his *Era of the French Revolution, 1715-1815* [1929], p. 384). (2) Illyrianism is an early eighteenth-century proto-nationalist movement with sixteenth-century roots (Ivan Gundulitch); it reached political germination during the late seventeenth century in the negotiations of George Brankovitch and Arsene III Chernoyevitch (see M. de Vos, *Histoire de la Yougoslavie* [1965], pp. 51-56). (3) The Serbs *did* hold at least three national congresses during the eighteenth century: 1744, 1769, and 1774 (see P. Gunst, *Magyar Történelmi kronológia* [1968], pp. 159, 164). (4) "Continental-minded" France is located in a coastal area of Europe, as any map will show. (5) An Ottoman foreign office *did* start emerging during the seventeenth century; by the eighteenth century, the *reis efendi* of the Porte's chancellery was playing the role of a foreign minister (see R. Mantran, *Histoire de la Turquie* [1968], p. 57). (6) Tsar Nicholas I was an antinationalist at home and a protagonist of nationalist rebels abroad; witness his suppression of the Polish revolt in his own empire in 1830 and his military-diplomatic support of the Greek insurgents on Ottoman territory during 1827-30 (see the index to any scholarly history of Russia, such as those by Pares, Vernadsky, Seton-Watson, or Riasanovsky). (7) The impact of the coming of capitalism and of its nineteenth-century crises on the rise of socialism in Middle Europe is proportionately discussed on pages 197-207 of my book. May I reiterate that my book is not a chronicle of middle-zone events but an inquiry into the historical processes that made Middle Europe what it is today.

Professor Meriag incompletely quoted my reference to the Kerner bibliography as "the standard bibliographic guide to Slavic Europe." He omitted my limiting qualification: "covering the pre-1918 period" (*Middle Europe*, p. 263). His use of the word

"Western" with reference to my bibliographic familiarity is a semantic puzzle. The American secondary literature the reviewer missed in my book is collectively cited in my opening reference on page 263 to American Historical Association Pamphlet no. 425, the bibliographic listings of which, incidentally, do not contain my up-to-date West European literature.

I would be grateful to have the reviewer's, or the Editor's, or any knowledgeable historian's comment on whether the facts I stated in my book and reiterate in this letter are or are not supported by the accompanying authoritative bibliographic references.

LESLIE C. TIHANY  
Fort Thomas, Kentucky

PROFESSOR MERIAGE REPLIES:

(1) The peace agreement signed at Vienna in 1809 is known generally as the Treaty of Schönbrunn. See *Encyclopedia of World History* (1968 ed.), 644, 645, 748; *Der Grosse Brockhaus*, 16: 770; and the works of Robert Kann, C. A. Macartney, Geoffrey Bruun, Charles Breunig, and Steven T. Ross. (2) In the introductory chapter of *Ljudevit Gaj and the Illyrian Movement* (1975), Elinor M. Despalatović briefly reviewed the controversy among Croatian scholars over the exact year in which the "Illyrian Movement, or the Croatian national awakening" began; none of the dates mentioned is earlier than 1832. Despalatović placed the date in December 1835, since "Gaj first enunciated the goals of the Illyrian Movement" at that time. (3) During the eighteenth century the Serbian nation was dispersed throughout a number of provinces in both the Austrian and the Ottoman Empires. The "national congresses" to which Leslie C. Tihany referred, without qualification, were actually assemblies convened, with the permission of the Habsburg government, by the Serbian Orthodox Church hierarchy of the Vojvodina (in southern Hungary). These assemblies were national only to the extent that they represented that segment of the Serbian nation residing in the Vojvodina. The majority of the Serbians, of course, lived south of the Danube under Ottoman rule. See Dimitrije Ruverac, *Srpsko narodno-crkveni sabori od 1690 do danas* [Serbian National Church Assemblies from 1690 to the Present] (1889). (4) France has sea coasts, to be sure, but this did not prevent it from developing a Continental psychology. France's Europe-centered *politique* has been a dominant feature of modern French history. Witness the compelling desire of successive generations of French leaders to establish French frontiers on the Rhine. A French general once remarked, "If you were to bring me all the empires of Asia and Africa . . . , they wouldn't in my eyes be worth an acre of earth where I fought in 1870." Another French officer wrote on the eve of



World War II, "In the event of a new world war, the fate of the French Empire, and in particular that of Indochina, will be settled in Europe or, to be more exact, on the Rhine." (5) The use of the term "Ottoman foreign office" for the seventeenth century suggests a very Westernized view of the Ottoman state. The *reis efendi*, according to Stanford Shaw, "acted as the private secretariat of the reis ul-kuttap, handling his correspondence, including that with foreign countries, thus being the closest equivalent to a foreign office" (*History of the Ottoman Government and Modern Turkey*, 1 [1976]: 128). (6) Nicholas Riasanovsky has said that Nicholas I "emerged as a relentless foe of nationalism on the international scene, while his treatment of nationalism at home appeared to be more complex and ambivalent. . . . At home nationalism often went hand in hand with such state interests as the suppression of the Poles or the integration of the Western Provinces into the Russian Empire" (*Nicholas I and Official Nationality, 1825-1855* [1959], 237-38). And C. M. Woodhouse quoted Nicholas I as follows: "I detest, I abhor the Greeks, I consider them as revolted subjects and I do not desire their independence" (*The Greek War for Independence* [1952], 127). (7) The pages that Dr. Tihany cited refer to his chapter entitled "The Coming of Socialism." I must respectfully disagree with his contention that the rise of capitalism is "proportionally" treated in these same ten pages. Tihany has evinced little appreciation for the genuinely revolutionary effects that increasing integration with the European market had upon the subsistence economies of "Middle Europe" during the course of the nineteenth century. "As an exceedingly dynamic and revolutionary economic system," Jozo Tomasevich has written, "capitalism conquered and transformed the traditional economic order. . . . The shift from a subsistence to the more market-oriented economy was by far the most important structural change which took place in the economy of these [South Slav] lands during the century preceding the First World War" (*Peasants, Politics, and Economic Change in Yugoslavia* [1955], 211).

I submit that the Kerner bibliography is not "still the standard bibliographic guide" for either the pre- or post-1918 period. The Kerner guide, while remaining a valuable and useful tool, was, after all, published in 1918. Yet Dr. Tihany has unaccountably ignored the two massive volumes edited by Paul L. Horecky, which were published in 1969, the result of a collaborative effort by a large number of leading scholars specializing in East European studies.

Moreover, Dr. Tihany indicates in his letter that the missing "American secondary sources" to which I called attention in my review have been "collectively cited" by his reference to "American Histori-

cal Association pamphlet no. 245." Is a reviewer—any reviewer—to assume that the citation of a general bibliography by an author indicates that all of the items in it were, in fact, consulted? The collective citation alluded to by Tihany is certainly no substitute for the actual sources and literature used. As a point of clarification, Aleksander Gieysztor's *History of Poland* is not an "American source" but rather a study prepared by five distinguished Polish historians and published in 1968 by Polish Scientific Publishers of Warsaw.

LAWRENCE P. MERIAGE  
University of Kansas

#### TO THE EDITOR:

Thank you for selecting my book *Etienne-Denis Pasquier: The Last Chancellor of France* for review (*AHR*, 82 [1977]: 1266). I appreciate the recognition granted the book and its subject by your decision. In his review David H. Pinkney not only questioned whether Pasquier, whom Pinkney called a "trimmer," is worthy of a biography but also doubted the appropriateness of what he called "a purely political biography."

Pasquier was indeed a trimmer, a man who survived politically from the Old Regime to 1848 by compromise. But as the reader will quickly discover for himself, Pasquier was also a competent, efficient administrator whose early career reflected his sensitivity to the social and political realities of his time. As prefect of police his keen awareness of the relationship between bread shortage and political and social upheaval contributed significantly to the tranquility of Paris during the later empire. The men Pasquier chose as judges and prefects at the Second Restoration and his role in the dissolution of the *Chambre Introuvable* in 1816 indicated a realistic assessment of the measures necessary to keep government functioning as well as a recognition of the shifts that had occurred in political power and public opinion. Pasquier's fears of both excessive royalism and excessive liberalism are clearly illustrated in my chapter on his term as foreign minister. In this capacity he delicately balanced the needs of foreign policy with the demands of domestic politics both in the cabinet and in the chambers. Unfortunately, however, by the 1830s Pasquier, now president of the Peers, had completely lost touch with these forces.

Professor Pinkney asked, "What ambitions drove him? What fears restrained him? What passions diverted him?" Clearly, Pasquier was driven by egotism and by a hunger for public office. But devotion to his heritage of the nobility of the robe is what my biography stresses as the most consistent and compelling motive in his life. No passion seems to have diverted Pasquier at any length, save that for Madame de Boigne, and extensive research in



Pasquier's own archives revealed no discernable influence by her on his career. But regardless of the motives that inspired him, all such elements must be considered in the context of his political life, for that was the career to which he devoted himself.

JAMES K. KIESWETTER  
Eastern Washington University

TO THE EDITOR:

In Laura Oren's review of my book, *Women and Trade Unions: An Outline History of Women in the British Trade Union Movement* (AHR, 83 [1978]: 1016-17), she stated that the chapters dealing with the pre-1920 period add little that is "original." I am unaware of any study dealing with the very important part played by Margaret Irwin in the foundation of the Scottish TUC before my own article on the subject appeared in the *Scottish Labour History Journal* in 1973, which together with other unpublished material from the National Association for the Promotion of Social Science and almost everything from *The Pioneer* and *Scottish Weaver's Journal* had not been used before in any published work. Nor had the material on the Women's Friendly and Benefit Societies of the late eighteenth and early nineteenth centuries and other items. All earlier works, including Barbara Drake's very valuable pioneer study and the TUC's own booklet to which Laura Oren referred, accept that women played virtually no part in labor history before the Women's Trade Union League came into existence in 1874. So far as I know, before my own book there has been no attempt to link the considerable activities of the women in craft guilds, shown in the medieval French, Latin, and English documents collected by Maud Sellers and others, and women's part in the labor protests of the sixteenth, seventeenth, and early eighteenth centuries to labor history. The fact that all of this was telescoped in the first two chapters does not invalidate it.

The distortions that have resulted from ignoring or belittling women's role in labor history are extensive, and it will take much work to set the balance right. Many detailed studies are necessary, as I have pointed out in the foreword to my book. I look forward to the publication of Laura Oren's own work on the pre-1920 period.

SHEILA LEWENHAK  
Ruislip, England

TO THE EDITOR:

I was surprised to see that my book, *The Origins of the Crimean Alliance*, was given to Michael M. Finefrock to review (AHR, 83 [1978]: 1247). Since Professor Finefrock's field is twentieth-century Turkish domestic history, it seems odd to have had him re-

view a book in the "Modern Europe" category. I myself agree with the editors that my book fits best in that section. If the choice had been to give the book to an Ottoman historian to review in the Near East section, I should not feel that I have grounds for complaint. But, since the book was reviewed as a contribution to modern European history, readers would naturally assume that the reviewer is a specialist in that field. To print a review written from a different perspective without warning the reader seems to me highly misleading.

Professor Finefrock's review, which was very unfavorable, includes a remarkable number of elementary errors. Several peculiar statements suggest his unfamiliarity with the context of scholarship on the Crimean War. He apparently took me to task for failure to refute the Gooch thesis, based on Brisson D. Gooch's article on Crimean War historiography in the *American Historical Review* in 1956, from which I quoted on the first page of my preface. In fact, there is, of course, no Gooch thesis. Gooch said that we know all of the facts we are likely to know about the origins of the war (the point I use), but Gooch also noted approvingly a remark of H. W. V. Temperley's to the effect that we still do not completely understand the origins of the war and devoted his article to outlining "several areas which await further investigation." Among other things, he suggested that "an accurate account of Redcliffe's activities at Constantinople [an important aspect of my project] would elucidate the entire picture." In fact, prior to 1977, there had been no book-length treatment in any language of the origins of the conflict since the mid-1930s. It is particularly strange, therefore, to find Finefrock deploring "still another inquiry into the war's origins."

Professor Finefrock also seems to have had trouble placing my interpretation within the scholarly debate. I did not, for example, offer "an apologia" for Stratford Canning; Temperley did that. I tried to refute Temperley. Although I agree with Temperley that Stratford probably did not deliberately incite the Porte to go to war, I did not base my conclusion (as did Temperley) on a lofty estimation of Stratford's character and understanding of the Ottoman scene. Instead, I suggested that Stratford's advice, which was often both arrogant and inappropriate, was only one of many factors weighed in Ottoman decisions and was probably less important than other considerations that I developed, such as the views of Ottoman traditionalists, distrust of Vienna, and highly colored reports from the Ottoman representative in London. A more careful reading, here and elsewhere, would have helped Finefrock. To note another small point among many, the "inconsistency in transliteration" that disturbed him was explained on the second page of

the preface. I did not, in any case, use the form "Unkiar Skellesi," which so far as I know does not exist anywhere outside his review.

Finally, Professor Finefrock seems to have been unable to grapple with the ideas I tried to present. His own suggestion that "Russian military evacuation of the Rumanian Principalities in July 1854 . . . altered the entire course of the war and brought it to the shores of the Crimea" hardly comes as a surprise to Crimean War specialists; it is, in any case, a point I discussed at some length in my conclusions. Those conclusions, which run to some ten closely argued pages, Finefrock virtually omitted from consideration; he merely took the last sentence out of context to prove that Saab had "an incomplete understanding of her topic," "eschew[ed] interpretation," and offered only "a concluding irrelevancy." Possibly he was waiting for me to administer the final blow to the Gooch thesis; if so, his confusion is understandable. The condescending note on which he ended the review ("Nothing really for Gooch to worry about") is hardly in keeping with the AHR's generally judicious style; coming from someone who is not even a specialist in the subject, it is, to say the least, disconcerting.

I may say that I have welcomed the AHR's policy in recent years of opening its review pages to a wider and younger group of reviewers. Surely, however, that policy can be more successfully implemented than it has been in this case.

A. P. SAAB  
University of North Carolina,  
Greensboro

#### TO THE EDITOR:

Since an author's revision of an earlier book often goes unnoted by his peers, I was pleasantly surprised that the editors found my recent revision of *Jacksonian America: Society, Personality, Politics* substantial enough to justify a review (AHR, 84 [1979]: 548-49). That a scholar of Richard P. McCormick's stature thinks that the revision "strengthens and amplifies" my earlier interpretation and that he praised what he was kind enough to call my "vast erudition," my prose style, and my reading habits, I find strangely unobjectionable—if not downright perceptive. That he disagreed with several of my views is similarly unobjectionable because such disagreement is inevitable. I take pen in hand to perform the generally fruitless and unexhilarating task of responding to a review not because Professor McCormick disagreed with this or that one of my arguments but because his review distorts my arguments. Had the surprisingly misleading and inaccurate comments in the review been written by someone dimwitted or patently inept I would shrug them off and go about my business. But since they

were written by a man as bright and as learned as Richard P. McCormick, I feel I have no alternative but to try to set the record straight.

Professor McCormick stated that I do "not seem to like anything about the Age of Jackson, least of all the Old Hero." Whether a historian likes the people and developments he discusses is neither here nor there; his job is to try to understand and explain them. Since McCormick of course knows this, I assume he meant to suggest that my animus prevented me from seeing any good in men and things deserving of praise.

I admit to harboring some reservations about "the Old Hero." Since I have never admired humbuggery, demagoguery, hypocrisy, egomania, contempt for constitutional precedents and law, or a penchant for violence and bluster in presidents, I do find little to praise in the public performance of Andrew Jackson. Yet I do manage to praise—in part because I like—so much about the era. A swift and partial catalogue of men and things *Jacksonian America* includes James Fenimore Cooper for his "penetrating intelligence"; Mrs. Trollope and Dickens for their honesty; Thomas Hamilton, J. S. Buckingham, and Harriet Martineau for their good sense; Tocqueville for his brilliance and originality (not for his lack of interest in *mere facts*!); American friendliness and humor; the growing opportunities for social advancement available to poor Irish immigrants; New England's comparatively fair treatment of free blacks; politicians who resisted the anti-Catholic trend; the era's educational reform, which I defended against modern critics, by writing, "that Jacksonian public schools were administered in accord with the philosophical ideas of the property-owning elements who financed them . . . detracts little from the worthwhileness of the era's educational reform" (p. 63); important achievements in American medicine and in the treatment of the insane; the era's vigorous economic growth, for all of its erratic nature; the good sense of many businessmen (I observed that "what is bad for business is not necessarily good for the country" [p. 258]); Democratic Party champions of prison reform; Thomas McKenney for much of his Indian policy; the Second Bank of the United States under Biddle; Louis McLane and William Duane for standing up to Jackson; Fanny Wright for her marvelous presence and her idealism; even Jackson himself, when he saw through flagrant placemen; Amos Kendall in administering the Post Office (if for little else!); Van Buren's moderateness in foreign affairs; Philip Hone for his honesty, his lovely handwriting (which makes his manuscript diary so legible), and his charming lifestyle; the Erie Canal (which I think was smashing!); Henry Clay for his deftness; John Quincy Adams for his crotchetyness and his integrity; the sound bankers of the era; Robert Rantoul

and Franklin Plummer, for their ability to speak plain truths; the sincere Whig humanitarians in New York and elsewhere; . . . but why go on? If amoral major party politicians and increasing inequality have not induced me to dance in the streets, I have remained sensitive to the beauties of more attractive people and happier impersonal trends.

According to Professor McCormick, my book views the differences between the Whigs and Democrats as "merely an electoral ploy" devised by party leaders who were "seeking pelf and power." Here McCormick distorted slightly but significantly what I said in the concluding paragraph of my chapter on the rise of the major parties in the states. In questioning some historians' belief that major party differences on issues constitutes proof that the parties were therefore not electoral machines that subordinated principles to success at the polls, I offered the following essentially logical argument: "No organized political group is ever a mere electoral machine. No matter how cynical and amoral its leadership, the presence of a powerful opposition leaves them no alternative in a democracy but to appeal to the electorate and to do so in terms of lofty principles that are distinguishable from the lofty principles proclaimed by their opponents. For even the most simpleminded and gullible voters are unlikely to respond favorably to a party naive enough to proclaim frankly its dedication to pelf and power" (p. 232). If, as I also observed, "the truths concerning Jacksonian [major] parties are more subtle than the contemporary partisan rhetoric or, for that matter, the scholarly controversy of a few decades ago might suggest" (p. 204), it is because I have found these parties most complex, whether in philosophy, policy, or action.

Although the burden of the mounting evidence is that most state party leaders were pragmatists who indeed appear to have subordinated principles to winning, holding, and using office in their parties' interests, my chapter reports interesting examples of high-minded men who occasionally prevailed, as among the Massachusetts Democracy and New York City Whiggery, and it notes too the ideological heterogeneity that was often the case within the state parties. A reader might never guess from Professor McCormick's review that *Jacksonian America* has passages such as the following: "the 'admirable attitude' [of many Whig leaders] on racial matters is neither conservative nor radical but that fact detracts not at all from the significance of the attitude. There are other ways of classifying beliefs, after all, than in the terms used by economic determinists" (p. 215).

Finally, Professor McCormick claimed that in my book "Jacksonian America was rigidly divided into two classes, the opulent rich and the miserable poor.

There was negligible mobility," and he asked, in closing, "Was the class structure that simple and rigid? Were there no redeeming virtues?" As my catalogue of things admirable attests, I found many "redeeming virtues" in Jacksonian America. Nor do I portray American society in the oafishly simplistic terms McCormick attributed to me.

The book indeed reports that modern quantitative studies by many scholars and me disclose that in all sections of the country the inordinate share of their communities' wealth commanded by a small wealth-holding elite became greater with the passage of time, while the proportion of persons assessed for no wealth whatever also increased. I note that "this does not necessarily mean that they [the latter] were either starving or penniless," since assessors characteristically accepted the untruths people told them about their net worth. If my discussion appears to emphasize the two groups at the opposing ends of the property spectrum, it is because the evidence is most abundant for them. I also discussed the middle class, emphasizing how little scholars know either about its size or its living standards. As for the farmers, who constituted the bulk of this class, I noted that, on the one hand, "during the antebellum decades ordinary farm families began to use or bring into their homes articles and implements earlier enjoyed only by the well-to-do" (p. 79), while, on the other, "random impressionistic" and therefore less than satisfactory evidence suggests "the monotony, the hard work, and the generally poor quality of life experienced by the nation's yeomanry during the era" (p. 83). As for labor, most modern economic historians find that its real wages declined during the era.

While *Jacksonian America* reports that upward mobility of the sort that catapulted men from lower echelons into the upper crust was not common, it also observes that "social movement into and out of more modest [social] categories is at least as important and may throw more light on the character of a society." Unfortunately, it continues, the evidence on the latter type of mobility is scattered ("neither abundant nor definitive"), "lighting up only a small portion of the antebellum social landscape." The book gives examples of rural New England farm boys aided by "institutional and financial support," who attended college in increasing numbers, winning degrees that offered them "a favorable alternative to the labor that would otherwise have been their lot." I also speculated "that the era's political leaders, like Philadelphia's lawyers, were increasingly men who had been born into the nation's plebeian orders" (p. 90).

If I have taken inordinate space in responding to Professor McCormick's contentions, it is because general assertions to the effect that I am not the humorless simplistic fellow these contentions make me

out to be would hardly be convincing to readers whose only knowledge of my book comes from McCormick's review of it. A man's reputation for intelligence or a lack thereof can be much more quickly attacked than it can be defended.

EDWARD PESSEN  
Baruch College, and  
The Graduate Center,  
City University of New York

PROFESSOR MCCORMICK REPLIES:

Edward Pessen's *Jacksonian America* is an important book that challenges many previous interpretations of the period. I do not believe that I distorted his arguments, but the final verdict on this issue will properly rest with our colleagues in the field.

RICHARD P. MCCORMICK  
Rutgers University

TO THE EDITOR:

June Sochen's comments on *Children of Fantasy: The First Rebels of Greenwich Village* (AHR, 84 [1979]: 576-77) offered only her own fantasies about Greenwich Village, disingenuously presented as a review. Sochen attacked my book for not adopting her unsupported assumptions about the 1910-20 Village and its Bohemian inhabitants. She characterized my conclusions as "superficial" without indicating possible "deeper" explanations, an omission that suggests, as does her editorializing, that her condemnation is ideological rather than analytical.

I hope that Professor Sochen's hostility was not aroused by my failure to cite her 151-page "seminal" work on Village feminism, which summarizes the activities and writings of five Village women. Since she condemned my collective biography of five Village men, I assume that she has simply applied the criticism leveled at her book to mine. While focusing on the careers of five important Villagers, my book assesses the contributions of both men and women to Village culture.

Professor Sochen asserted that I chronicled the "intellectual history of a movement or a school of thought," which I did not, though I did examine the assumptions, views, and values of the Village rebels. My book demonstrates that there was neither a "general philosophy" nor a coherent program. The ease with which psychoanalysis, socialism, progressive education, feminism, cubism, and free love were tossed together without heed to contradictions was an indication of intellectual confusion and the importance of rebellion for its own sake.

Professor Sochen complained that my study lacks sufficient "connection" because it "does not focus

upon the shared philosophy." She suggested that I could have "made much" of the "fact" that "four of the five" attended Harvard. Only three of the five went to Harvard, and George Cook was there only one year. Since Hutchins Hapgood received his B.A. in 1892 and John Reed in 1910, I will let Sochen work on that insight.

The connective link is not in a "shared philosophy" but in the prevalence of similar personal characteristics. Each individual experienced a lonely childhood that caused him to construct rich fantasy worlds in which he preferred to dwell, protected from threatening external realities and deep feelings of inadequacy. Professor Sochen commented disdainfully that the pursuit of romantic dreams is "hardly a novel problem," implying that what is common is insignificant. Because these five rebels came from such disparate backgrounds, an understanding of their personalities is essential to explain the move to Greenwich Village. All five wished to escape ordinariness and become important individuals in league with makers of history or forces of upheaval that would alter the social order. But they were more interested in being identified with a movement than in attaining its ends.

Believing that there was once an intellectual Camelot, Professor Sochen seems to have been unable or unwilling to perceive the narcissistic underpinnings of the cultural revolt. Her beliefs and writings about Village culture conflict with my findings. Failing to disclose this conflict to the reader, Sochen castigated the book for its failure to meet her gratuitous assumptions about Greenwich Village.

ROBERT E. HUMPHREY  
University of Iowa

TO THE EDITOR:

Victor S. Mamatey's comments on my book, *Czechoslovakia's Role in Soviet Strategy* (AHR, 84 [1979]: 211-13), are a historians' tour de force and an exercise in selective perception rather than a review of the work. He wrote very little about the study itself, and part of what he said is inaccurate.

First, Professor Mamatey claimed that the title of the study "is somewhat misleading" because, as he put it, the book deals with "Czechoslovak policies and attitudes toward Russia and the Soviet Union from the First World War to 1968." This assertion would seem to indicate that he has overlooked the main part of the work that "provides important keys to the understanding of the skillful strategy the Soviets are executing at present on several continents," to use the words of a well-known authority on international relations and Soviet strategies.

The reviewer quite obviously ignored the introductory chapters dealing with the Marxist-Le-



ninist strategy and tactics since the October Revolution and the chapters entitled "A Model for Revolution" and "Czechoslovakia's Role in Soviet Strategy," which deal specifically with Czechoslovakia's role in Soviet long-term strategy against the background of the theory and practice of Marxism-Leninism, in terms of Soviet interests, and in the broad context of international relations. It is shown in these chapters how the Soviets have used Czechoslovakia as a proxy in espionage, propaganda, and international politics and to what extent the Czechoslovak "laboratory experiment" is applicable to such countries as Cuba, Chile, Italy, and Portugal.

Second, the author is not "a Czech political exile in the United States" who approached his subject "from the point of view of the Czech political Right." I do not belong to any exile organization, am not active in exile politics, and am not concerned with "left or right." I search for truth. Had Professor Mamatey looked up biographical data on me, listed in the book, he would have had to notice that I am an American citizen, was educated in the United States, and have lived in this country longer than I lived in Czechoslovakia. I have taught history and political science since 1957. I could not possibly belong to the Czech political right because of my age and the fact that the National Front government in Czechoslovakia had already proscribed the political right and center in 1945. In addition, no member of my family has ever belonged to the political right in Czechoslovakia before or during World War II.

Third, had the reviewer read the book carefully, he would have had to notice that in no place did I suggest that the Soviets have established their domination over Czechoslovakia or other countries in East Central Europe "as a result of this or that treaty." On the contrary, I stated that not merely the treaty but the establishment of the National Front government in April 1945 "did not automatically seal the fate of Czechoslovakia. In wartime, fundamental decisions are usually made on the battlefields; and the larger part of the country was still controlled by the Germans" (p. 199). Extensive documentation on communications regarding the importance of the occupation of Prague by the Western armies between Winston Churchill on the one hand and Roosevelt, Truman, and Eisenhower on the other follows that assertion.

Fourth, the reviewer's claim that the book "fails completely" to present "the Czechoslovak problem in the context of great power relations" cannot be documented. The opposite is true. My book discusses in great detail French, British, German, and Soviet interests in and policies toward Czechoslovakia.

Fifth, Beneš was not clairvoyant and, therefore,

in 1943 could hardly have known that "the British and Americans were not going" to Prague. His reasons for the decision to sign the treaty with the Soviet Union and his preference for the Soviet, rather than the Western, occupation of Czechoslovakia are explained in chapter 8, "Beneš and the Question of East Central Europe."

JOSEPH KALVODA  
*St. Joseph's College,  
West Hartford*

#### PROFESSOR MAMATEY REPLIES

I should like to respond only to one point in Joseph Kalvoda's rejoinder to my review of his book, namely, his objection to being called a "Czech political exile" who approached the subject "from the point of view of the Czech political Right." I am sorry and apologize if I have offended him by calling him a political exile. There was no intent on my part to offend him. He is a native of Czechoslovakia, which he left after the Communist takeover in 1948. The term "political exile" is not an epithet of opprobrium. As a matter of fact, it is a badge of honor in many countries, including Czechoslovakia, many modern leaders of which (for example, Masaryk, Beneš, and Štefánik as well as Šmeral, Gottwald, and Dubček) have been political exiles.

I do not know to which side of the Czech political barrier Professor Kalvoda belonged—if, indeed, he belonged to either side—and have not said so in my review. I must, however, reiterate my statement that he has approached the subject from the point of view of the Czech political right. He has done so both in general and on specific issues. He was quite critical of President Masaryk, the great prophet of the Czech democratic left, and has excoriated his presidential successor and political heir, Edvard Beneš, on page after page. He voiced no similar criticism against Masaryk's rival and adversary, Karel Kramář, the Grand Old Man of the Czech democratic right. To cite one concrete example, in the great historical controversy over the aims and tactics of the Czechoslovak movement for independence in World War I, Kalvoda's study supports the view that Masaryk had erred when he tried to keep the Czechoslovak Legion out of the Russian civil conflict in 1917 instead of leading it in a crusade against the Bolshevik government. This was Kramář's classical contention and that of his followers on the political right through most of the existence of the First Czechoslovak Republic.

As for the rest of Professor Kalvoda's book and my review, let the reader judge.

VICTOR S. MAMATEY  
*University of Georgia*



## ERRATA:

In Roberta Frank's review of Alfred P. Smyth's *Scandinavian Kings in the British Isles, 850-880* (*AHR*, 84 [1979]: 135-36) the name Ragnarr was incorrectly spelled Ragnaar. The error was the fault of the *AHR*, not of the reviewer. We regret the error.

THE EDITORS

In his review of Louis L. Snyder's *Roots of German Nationalism* (*AHR*, 84 [1979]: 481-82), Boyd C. Shafer wrote of Bismarck as "not an absurd racist." In the editing process, the last word was incorrectly copied as "socialist." We apologize for the error.

THE EDITORS

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## Recent Deaths

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RUSSELL I. CALDWELL, emeritus professor of American history, University of Southern California, died May 23, 1979, of congestive heart failure following a stroke. Born in Farrell, Pennsylvania, August 13, 1904, he received his B.A. from Hiram College (1927) and the M.A. (1933) and Ph.D. (1947) from USC. After sixteen years teaching and leading winning debate teams at Wooster High School in Pennsylvania, he joined the USC faculty as a lecturer in 1945 and steadily rose in rank, being promoted to full professor in 1964. He was a superb classroom teacher and always commanded large numbers of students in his courses. His special field of interest was the Federalist Era and U.S. Constitutional history. From his retirement in 1972 until felled by a stroke, he taught part-time in the USC school of journalism. He is survived by his wife, son, daughter, and two granddaughters.

DOYCE B. NUNIS, JR.  
*University of Southern California*

REID BINGHAM DUNCAN, professor of history at Emory University, died January 21, 1979. Born in Athens, Alabama on December 2, 1911, he grew up in Mississippi, where his father was a well-known Methodist minister. He received the bachelor's degree (1932) and the master's degree (1933) from the University of Mississippi and the Ph.D. degree (1938) from Cornell University, where he wrote a dissertation under the direction of Paul Gates. He served a year as a historian at Yorktown with the National Park Service, taught for two years (1939-1941) at Westminster College in Pennsylvania, and in 1941 went to Emory. Except for service in the United States Army (1943-1946 and again 1952-1954) and for a Fulbright lectureship at the National Chengchi University in Taiwan in the year 1959-1960, he spent the rest of his career at Emory. There he was an exemplar of service to his department and to the university.

His scholarly interests centered in American diplomatic history; and his courses not only demonstrated his mastery of fact, generalization, and sources, but also attracted many students for more

than thirty years. His articles dealt with American efforts to promote sales of agricultural products in European markets, and his book *Whitelaw Reid* (1975) was a skillful biography of a man whose career embraced journalism, politics, and diplomacy. The book received a Frank Luther Mott Award from Kappa Tau Alpha in 1976. Although a man of dignity and of much reserve with regard to self, Dr. Duncan was exceptionally generous of time and counsel with students and with colleagues, especially during the last six years of his life when he was director of graduate studies in history at Emory.

JAMES RABUN  
*Emory University*

C. BICKFORD O'BRIEN (1909-1979) pioneered in developing liberal studies on the Davis campus of the University of California and epitomized the ideal of the gentlemanly and humane teacher-scholar.

Bick O'Brien was born in San Francisco, earned his baccalaureate degree at Stanford University in 1930 and went on to U. C. Berkeley for graduate study in Russian history. He worked principally with Robert J. Kerner, James Westfall Thompson, and Raymond J. Sontag. He received a master's degree in 1934 and the doctorate in 1942. He then enlisted in the historical section of the U. S. Army Air Force, ending his service in 1946 with the rank of captain. After a year with the history faculty of Wellesley College (1946-1947) he joined two other colleagues in the Division of History and Political Science at the Davis branch of the University of California College of Agriculture. The Davis campus was then still known informally as the "University Farm" where faculty in the humanities taught an intimidating array of classes.

From 1947 to his retirement in 1974 O'Brien served several tours of duty as department chairman and was a leader in Academic Senate affairs. He was a calm and tactful force working for the creation of an outstanding College of Letters and Science and Department of History on the Davis campus. He contributed significantly to the civic

growth of Davis and to the work of the scholarly community of historians on regional, national, and international levels. He taught a variety of courses, and few others have been more conscientious or worked harder to give students what they needed. He set an example of cheerfulness, courtesy, and grace while insisting upon the students' intellectual development. As chairman of the Davis history department during a period of some growing pains, O'Brien was especially sensitive to ethical values and was warmly interested in the welfare of junior colleagues. Throughout all his activities shone a positive and undefeatable loyalty to his students, his colleagues, and the institutions with which he was associated.

Bick O'Brien's scholarly work focused upon seventeenth century Russian history; in this field he became an international authority. His meticulous research and careful generalizations illuminated the complex process by which Muscovy prepared itself in that century to become a major power in Europe. O'Brien's publications were marked by clarity of analysis and a tireless search for elusive sources. Standing out are two works, *Russia Under Two Tsars: The Regency of Tsarevna Sophie Alekseevna, 1682-1689* and *Muscovy and the Ukraine, 1654-1667*. Several periods of research in the Soviet Union as a participant in the US-USSR cultural exchange, combined with frequent visits to archives in Western and Central Europe, strengthened the authority of his numerous scholarly publications and contributed a fund of stories and thoughtful impressions with which to enliven his courses and deepen his students' appreciation for Russian history.

O'Brien participated generously in the affairs of historical societies. He held offices in the Pacific Coast Branch of the American Historical Association and the American Association for the Advancement of Slavic Studies, and was president of the Western Slavic Association. He also was prominent in the direction of the Yolo County Historical Society and the Fort Ross Interpretive Association. His work in the Association in recent years led to the publication in 1978 of an illustrated history of the Fort Ross area, *Fort Ross: Indians, Russians, Americans*, for which he was the general editor.

He will be missed, both as a civic leader and as a historian.

JAMES H. SHIDELER  
*University of California,  
Davis*

Born in Tallinn, Estonia, on August 21, 1924, VSEVOLOD SLESSAREV died of Alzheimer's disease on September 6, 1978, in Cincinnati, where he had taught

history for fourteen years. A social and economic historian whose theme was the expansion of Europe in the later middle ages, Slessarev was respected for scholarship of the highest quality and was loved for his great-hearted humanity. Although he did not live to complete his major studies of Genoese society and trade, his historical vision, his scholarly method, and his personal character remain as an inspiration for his many friends, colleagues, and students.

While Slessarev was in his teens, his native Estonia was annexed, first by the Russians, then by the Germans. The end of World War II found him a displaced person in Germany. He studied at the University of Hamburg and in 1950 emigrated to the United States, where he eventually became a citizen. The circumstances of his immigration largely determined the course of his new life. Because his sponsor was in Cincinnati, he settled there and resumed his studies. In 1952 at the University of Cincinnati, he completed a master's thesis on Genoa and Byzantium under the direction of Hilmar C. Krueger. That summer he married Helga Rettich, of Hamburg. During the final year of her doctoral studies in German at Cincinnati, he worked as an accountant rather than continuing his academic career.

Like most of Krueger's students, however, Slessarev soon went on to the University of Wisconsin in Madison. For four years he worked toward the doctorate under Robert L. Reynolds, who continued the Wisconsin tradition of medieval Genoese studies begun by Eugene H. Byrne. Few graduate students have come so well equipped in languages: Latin and Greek, German and English, French and Italian, Portuguese and Spanish, Russian and Old Church Slavonic, as well as his native Estonian. Fewer still have borne such learning so lightly and have applied it so diligently as did Steve—for he simplified Vsevolod thus for his American friends. In preparation for a dissertation on Genoese craft guilds, he read several thousand photostatic copies of commercial documents in the chartularies of medieval Genoese notaries. Under the tutelage of Gaines Post, he mastered the intricacies of medieval Roman and Canon law. He also held teaching and research assistantships in both ancient and medieval history and formed many warm, lifelong friendships. Already his scholarship was informed with a methodical serenity: always cautious and slow to judge, he would survey every facet of a subject, proceeding with painstaking deliberation, before coming to a masterful conclusion. Appropriately, at this period in his life the works of Leopold von Ranke occupied the place of honor on the mantelpiece of his apartment.

Before his dissertation was done, Slessarev moved

to Minneapolis in 1957, where he was employed for four years as research assistant for the James Ford Bell Collection in the University of Minnesota Library. Here, surrounded by rare materials for the history of voyages and explorations, gathered with a special emphasis on trade, Slessarev served a second apprenticeship under John Parker, curator of the collection. Again, Slessarev's self-imposed standards surpassed all expectations. He was asked to prepare a souvenir booklet for the annual banquet of the friends of the library; as in the past, it was to be a brochure of about twelve pages, containing a facsimile reproduction of some rarity from the collection, together with a brief commentary (*Speculum*, 32 [1957]: 603-07). The result, however, proved to be a substantial book, *Prester John: The Letter and the Legend* (1959), which in 127 pages not only surveyed that figure in history and historiography but also made notable original contributions of its own. When Robert Reynolds received a copy, together with the author's apology for being so preoccupied that his doctoral dissertation had made little progress, Reynolds replied by telephone: "Steve, you've just published your dissertation." Wisconsin duly awarded him a doctorate in history the next year, but he always explained with a modest smile, "I got my degree by accident."

In 1961, Dr. Slessarev returned to the University of Cincinnati, where he taught history until his retirement in 1975, attaining the rank of full professor in 1970. As a teacher he presented a distinguished and successful series of fresh courses that were notable alike for imaginative conception and for clear and pointed examples. His interest and range is reflected by topics that he set for theses and dissertations: crusading propaganda, missions to the Mongols, western expansion of the Slavs, Saint Brendan's voyage and the Vikings, Genoese impact of the Slavic world, and so forth. Two students completed doctorates under his direction: Frank Frankfort (1973) and Gregory Guzman (1968). By his graduate students, Slessarev is remembered as an indefatigable teacher/scholar who was nonetheless always available to them—at his office, at the library, and at home.

Slessarev was also active and honored in the historical profession. He held memberships in all the American professional societies that touched on his interests in history, and helped to found two more—the Midwest Medieval Conference and the Society for the History of Discoveries, of which he was elected president in 1967 and 1972, respectively. At the University of Cincinnati he was responsible for establishing the Committee on Medieval Studies; he also served as its first chairman. Characteristically, his greatest professional service was a work of piety; for he bore the heaviest share of

the editorial burden that produced a festschrift for his mentor, Robert L. Reynolds, in 1969. (David Herlihy, Robert Lopez, and Vsevolod Slessarev, eds. *Economy, Society, and Government in Medieval Italy* [1969]).

Others have written more in a single field but few have written with such authority in so many specialties as Slessarev. Broadly speaking, he worked on the one hand in medieval economic and social history and on the other in the history of explorations and discoveries, including cartography. The marvel is that none of his works overlaps another and that he pursued his research in both areas simultaneously.

At the same time, Slessarev never slackened his original interest in Genoa. Perhaps his finest work appeared first in German and demonstrated, through the etymology of surnames, that the so-called "Easterners" whom Eugene Byrne had identified in twelfth-century Genoa were in fact from southern France. ("Die sogenannten Orientalen im mitteralterlichen Genua: Einwanderer aus Südfrankreich in der ligurischen Metropol" in *Vierteljahrschrift für Sozial- und Wirtschaftsgeschichte*, 51 [1964]: 21-65). Less technical but of equal interest was a study of the way in which medieval merchant colonies grew up around the churches assigned to aliens resident in foreign cities. ("Ecclesiae mercatorum and the Rise of Merchant Colonies," in *Business History Review*, 41 [1967]: 177-97). He contributed "The Pound-Value of Genoa's Maritime Trade in 1161" to the festschrift for Robert L. Reynolds, mentioned above. His last, unfinished, work "The Genoese Colonies and the Crusades," was requested by Kenneth M. Setton as a chapter for the multi-volume work, *A History of the Crusades*. In search of sources for this chapter, in 1973 he had traveled the Near East as far as Trebizond. Eventually he had hoped also to return to the dissertation he never wrote, and which he tentatively titled "The Lower Bourgeoisie in Medieval Genoa, 1154-1257." The loss of these works is all the more to be regretted because he was uniquely qualified to write them.

Among the monographs published by Slessarev, his study of Prester John was followed by "Raphael Meffe's Contribution to the history of Portuguese Discoveries" (*Actas: Congresso Internacional de Historia dos Descobrimentos*, [1960]: 551-575). Then, in the later 1960s, he became involved in the Vinland Map controversy, to which he and an M.A. student contributed "The Vinland Caption Reexamined" (*Terrae Incognitae*, 1 [1969]: 58-67). His work on the history of discoveries was recognized both by a fellowship at the John Carter Brown Library in 1963 and by a prize award in 1968 from the Newcomen Society in North America.

Sincere and sympathetic to all, Steve Slessarev

was a devoted son, husband, father, and friend. His manner was gentle, gracious, and kind, with soft wit and a touch of melancholy. Ever swift to praise and slow to reprove, only dishonesty or cruelty could arouse his formidable anger. His complete integrity, his modest dignity and mild humility remind us of the saintly Pierre in *War and Peace*. He was in the fullest and noblest sense a humanist.

RICHARD D. FACE  
*University of Wisconsin  
Stevens Point*

GREGORY GUZMAN  
*Bradley University*

RICHARD KAY  
*University of Kansas*

LOUISE BUENGER ROBBERT  
*University of Missouri,  
St. Louis*



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# American Historical Association

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Founded in 1884. Chartered by Congress in 1889  
Office: 400 A Street, S.E., Washington, D.C. 20003

President: John Hope Franklin, *University of Chicago*  
President-elect: David H. Pinkney, *University of Washington*  
Executive Director: Mack Thompson  
Assistant Executive Director: Edmund H. Worthy, Jr.

**MEMBERSHIP:** Persons interested in historical studies, whether professionally or otherwise, are invited to membership. The present membership is about 14,500. Members elect the officers by ballot.

**MEETINGS:** The Association's annual meeting takes place on December 28-30. The meeting in 1979 will be held in New York, New York. Many professional historical groups meet within or jointly with the Association at this time. The Pacific Coast Branch holds separate meetings on the Pacific Coast and publishes the *Pacific Historical Review*.

**PUBLICATIONS AND SERVICES:** The *American Historical Review* is published five times a year and sent to all members. It is available by subscription to institutions. The Association also publishes its *Annual Report*, the *AHA Newsletter*, a variety of pamphlets on historical subjects, the bibliographic series *Writings on American History*, and *Recently Published Articles*. To promote history and assist historians, the Association offers other services, including an Institutional Services Program and the quarterly publication of the *Employment Information Bulletin*. It also maintains close relations with international, specialized, state, and local historical societies through conferences and correspondence.

**PRIZES:** The *Herbert B. Adams Prize* of \$300 awarded annually for a first book in the field of European history. The *Troyer Steele Anderson Prize* awarded every ten years to the person whom the Council of the Association considers to have made the most outstanding contribution to the advancement of the purposes of the Association during the preceding ten years (next award, 1980). The *George Louis Beer Prize* of \$300 awarded annually for a book on any phase of European international history since 1895. The *Albert J. Beveridge Award* of \$1,000 given annually for the best book on the history of the United States, Canada, or Latin America. The

*Albert B. Corey Prize*, sponsored jointly by the AHA and the Canadian Historical Association, of \$2,000 awarded biennially for the best book on the history of Canadian-American relations or the history of both countries (next award, 1980). The *John H. Dunning Prize* of \$300 awarded in the even-numbered years for a book on any subject relating to American history. The *John K. Fairbank Prize in East Asian History* of \$500 awarded in the odd-numbered years. The *Leo Gershow Award* of \$1,000 awarded in the odd-numbered years for the most outstanding work in seventeenth- or eighteenth-century European history. The *Clarence H. Haring Prize* of \$500 awarded every five years to that Latin American who has published the most outstanding book in Latin American history during the preceding five years (next award, 1981). The *Howard R. Marraro Prize* in Italian history awarded annually and carrying a cash award of \$500. The *James Harvey Robinson Prize* for the teaching aid which has made the most outstanding contribution to the teaching of history (second triennial award, 1981). The *Robert Livingston Schuyler Prize* of \$500 awarded every five years for the best work in modern British and Commonwealth history (next award, 1981). The *Watumull Prize* of \$1,000 awarded in the even-numbered years for a work on the history of India originally published in the United States.

**DUES:** For incomes over \$30,000, \$40.00 annually; \$20,000-29,999, \$35.00; \$15,000-19,999, \$30.00; \$10,000-14,999, \$20.00; below \$10,000 and joint memberships, \$10.00; associate (nonhistorian) \$20.00; life \$650. Members receive the *American Historical Review*, the *AHA Newsletter*, the program of the annual meeting, and the *Annual Report* on request and may subscribe to the *RPA* for \$7.00.

**CORRESPONDENCE:** Inquiries should be addressed to the Executive Director at 400 A Street, S.E., Washington, D.C. 20003.

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# American Historical Review

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Founded in 1895

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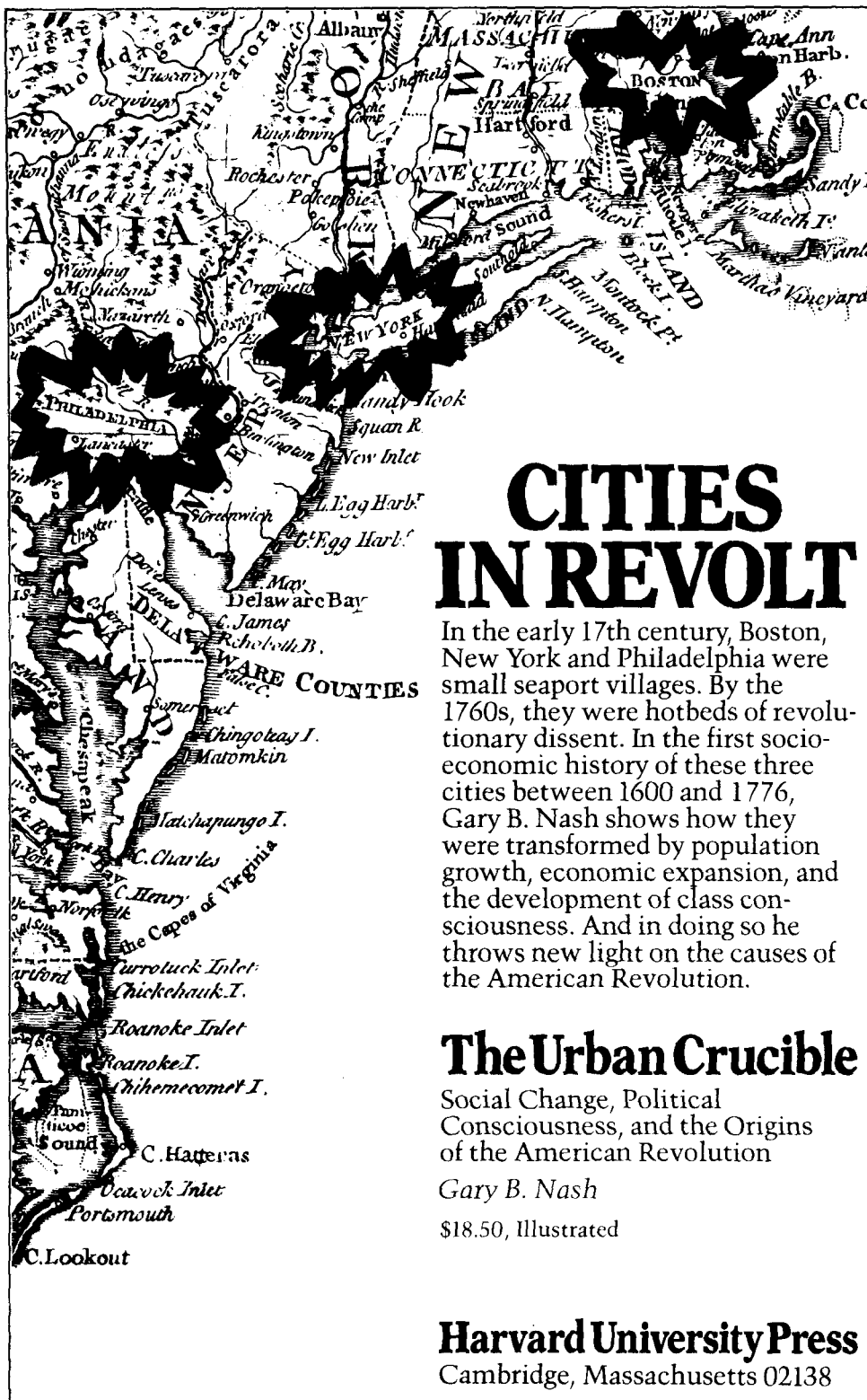
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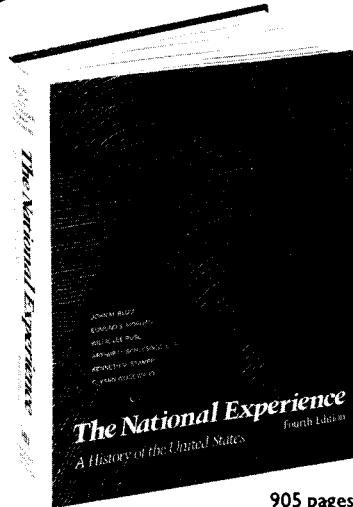


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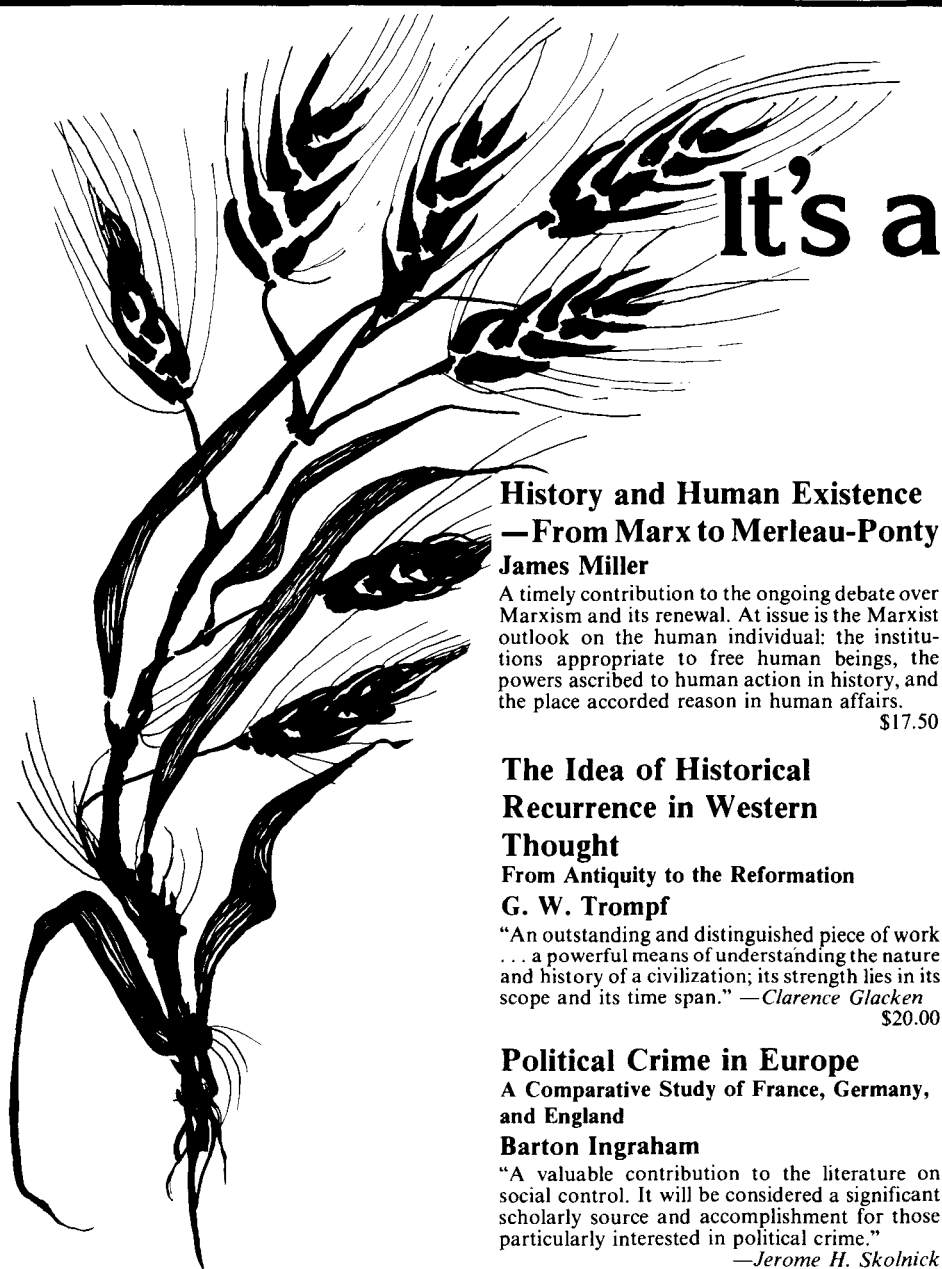
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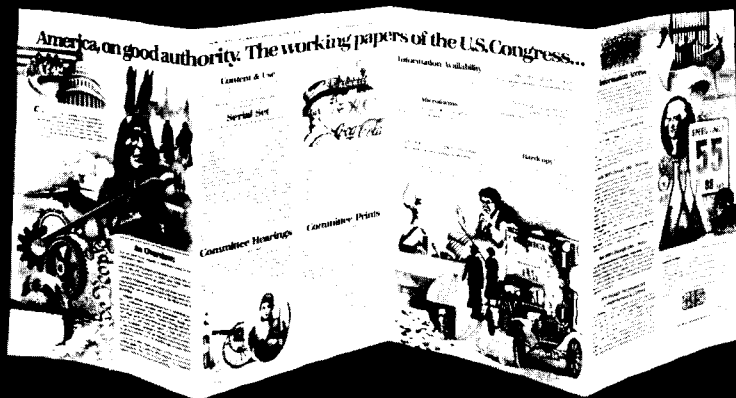
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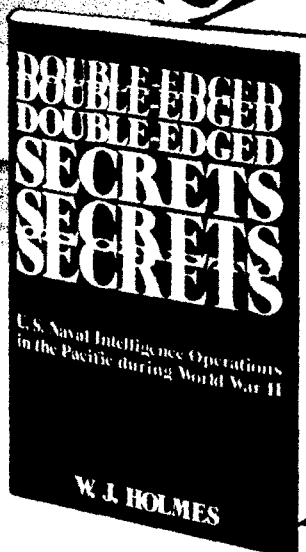
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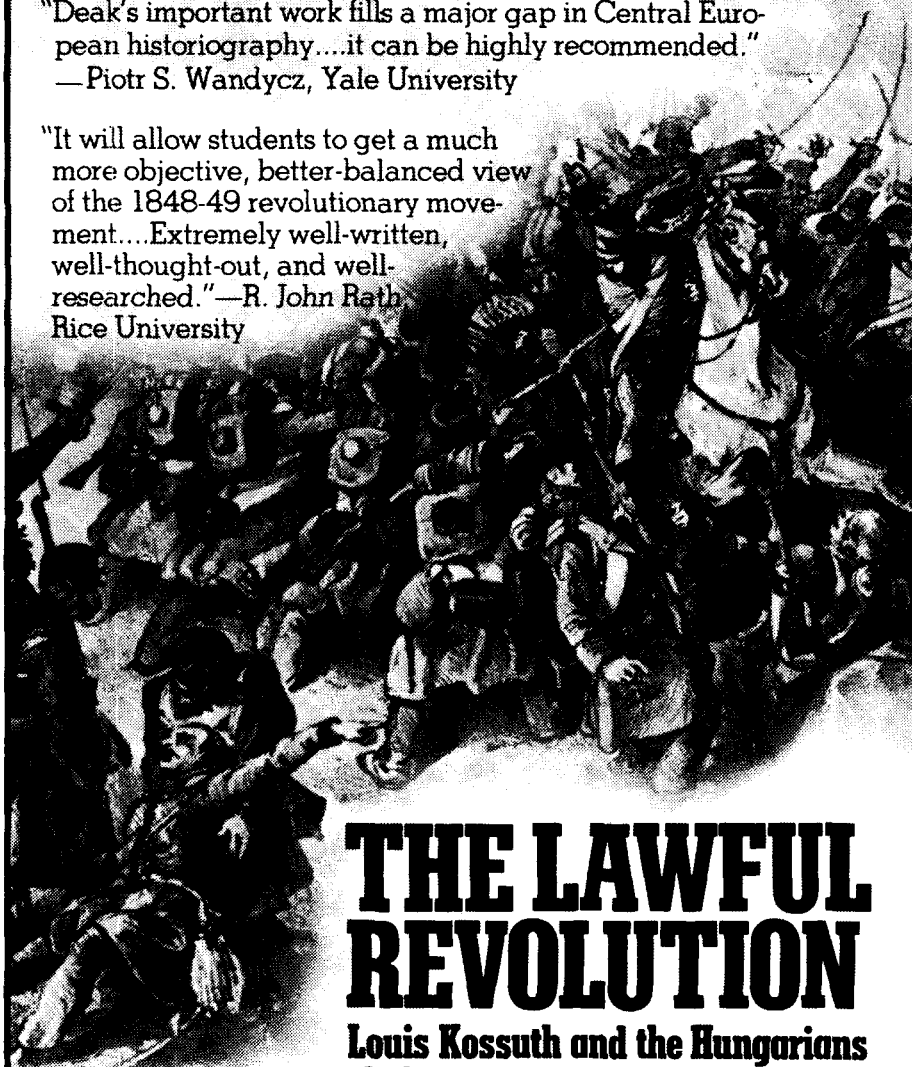
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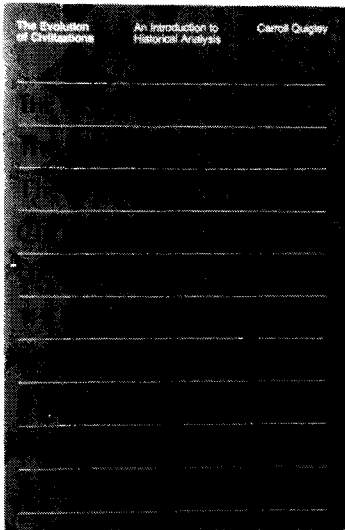
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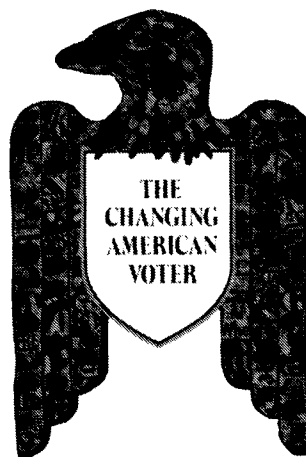
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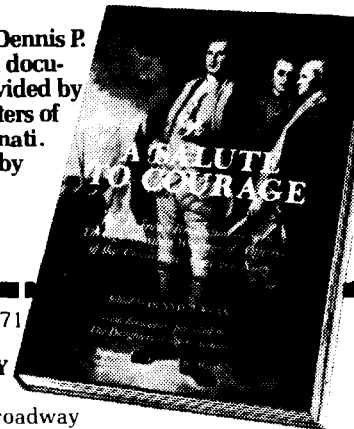


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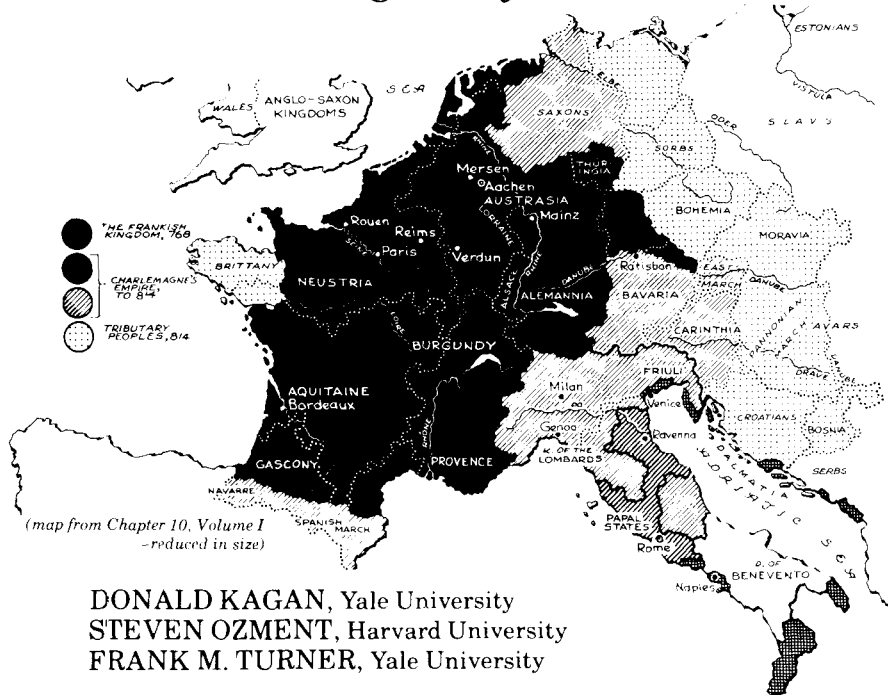
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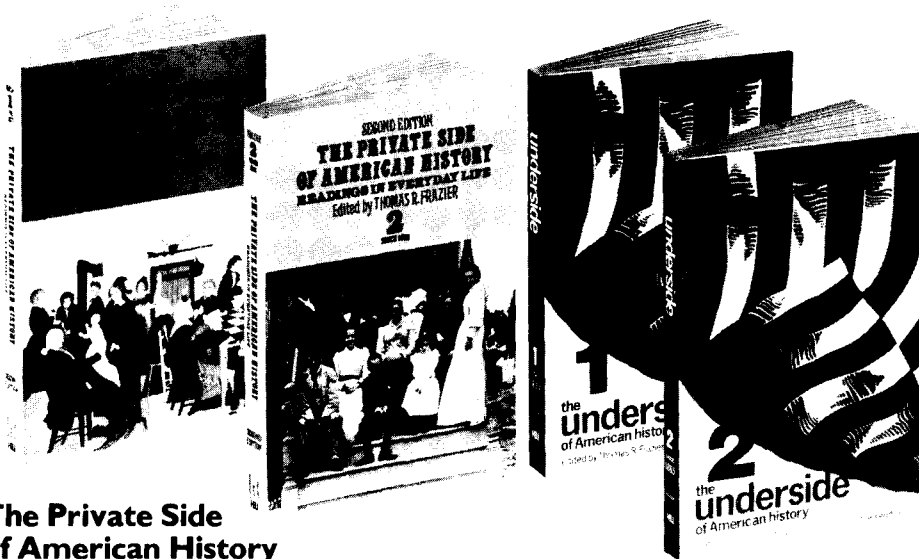
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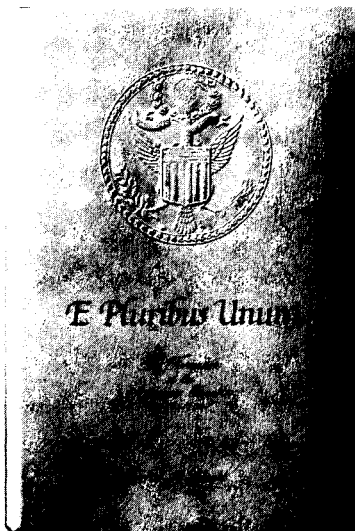
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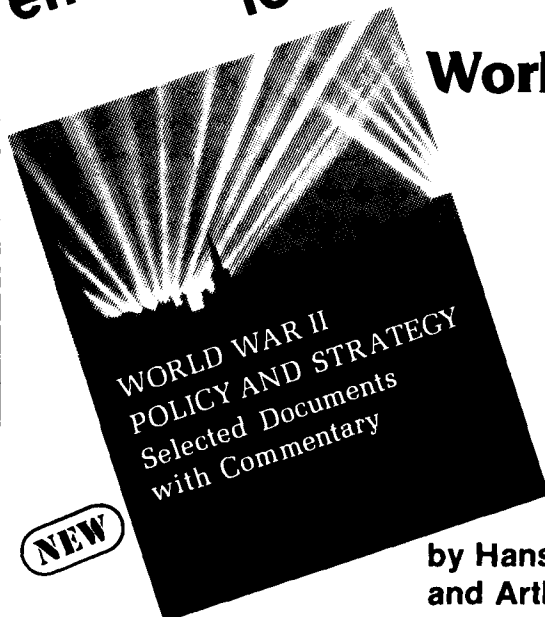
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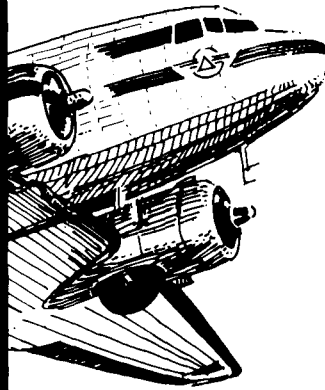
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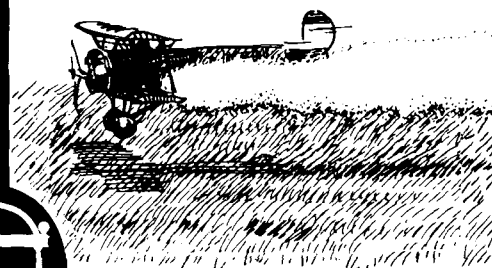
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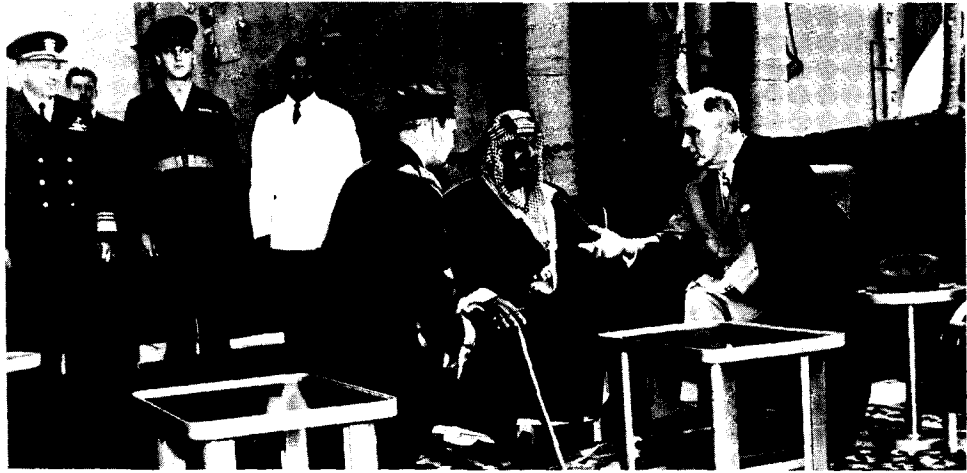
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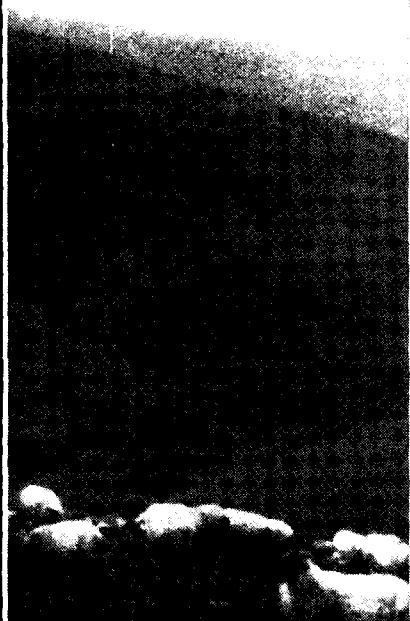
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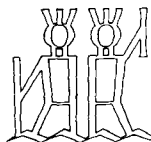
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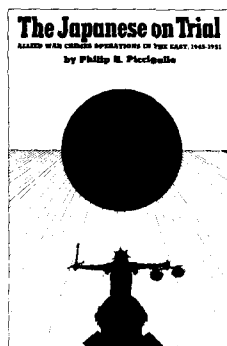
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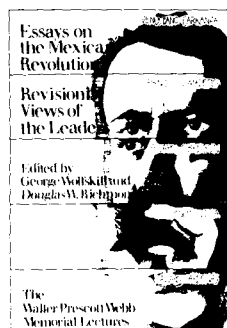


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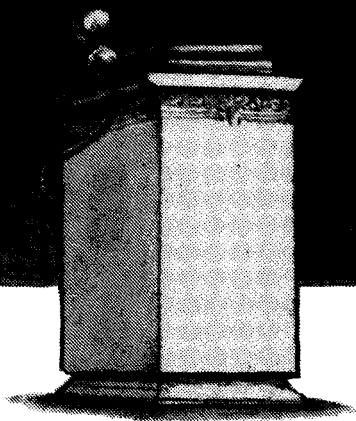
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